THE AMERICAN JOURNAL OF PSYCHOLOGY

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Vol. XLI

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1929

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THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded in 1887 by G. Stanley Hall
Vol. XLI JANUARY 1929 No. 1

ATTENTION-AUTOMATIZATION: AN INVESTIGATION OF THE TRANSITIONAL NATURE OF MIND[1]

By Adelbert Ford, University of Michigan

I. Experimental Aims

One occasionally hears the caustic comment that research is conducted in order to discover proof for preconceived theories rather than to formulate principles after the empirical evidence has been attained by strict laboratory methods. The researcher must be careful lest he be found among the dogmatists, who allow their biases to control their experiments. But it is evident that experimentation without a planned attack, without at least some warning of possible hypotheses to be developed, leads nowhere. The antecedent formulation of psychological theorems must be held as a scientific sin only when the worker is not frankly open to both favorable and unfavorable evidence.

The working hypotheses for our experiment were developed from a critical review of certain historical experiments on the nature of that supposed psychological state or process to which most texts refer as *attention*. More precisely stated, we are indebted to the work of J. J. B. Morgan[2] on distraction. Although terms, no matter how evasive, tend to commit one to conventional interpretations, we wonder to what extent Morgan's evasion of the word *attention*, or of any other term which signifies a theory,

*Accepted for publication March 7, 1928.

[1] From the Department of Psychology, University of Michigan.

[2] J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 5, 1916, (no. 35), 1-84.

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may have prevented an arrangement of experimental data, well done from the standpoint of laboratory technique, from showing certain interesting principles of mental integration.

Although the term *attention* has been attached, in most of the historical researches, to a description of consciousness, there seems to be no great difficulty in showing attributes of behavior which are comparable to the structuralistic descriptions. Certainly it is not impossible to give Titchener's definition of the focal point of consciousness a strictly behavioristic formulation. One need not observe an animal for a very long period before seeing fairly definite indications of 'focal' and 'background' coördinations. Neither does it seem unreasonable to hold that there are definite indications of a sequence of acts, determined by previous environmental conditions, which evolve from each other in a selective order. Nevertheless, the dominance of a coördination and the corresponding suppression of acts which are not serving the same end as the coördination seem to be a purely relative matter, for there are types of coördination which interfere with each other to a minor extent only, and others which seem to be thoroughly antagonistic. An examination of the life-history of these coördinations appears to reveal something in the interference which is dependent upon a lack of repetitive use.

For example, one finds when learning to drive an automobile that it is practically impossible to perform the several acts which are required in a sudden emergency without a great amount of interference. Although each of the acts is consummated easily enough when carried out independently, simultaneity is impossible. When one has learned to drive the car well, however, one can actually perform the various acts of stopping with perfect synchronism while conversing. The mere fact of use seems to bring about a complete cessation of mutual interference and it may also give indications that the various components of the coördination are more efficient when carried out in the presence of each other.

The interference of one stimulus against another is so common an occurrence in everyday life as to suggest one of the most urgent problems concerning the 'process' nature of mind in the sense of a transition from new syntheses to habituated coördinations—from attention to automatization. We hear much about the structure of consciousness or the classification of behavior and very little about the transitional nature of mind; although many experiments have contained the principles in a mass of data better arranged to conceal than to demonstrate.

The history of work on distraction therefore prompts the following theorems.

(1) We may not frame any hypothesis in such a manner as to invalidate the known facts of nervous functions. The general subject of the acquisition of neural integrations may, when descriptive of mental actions or operations, be referred to under the

general phrase *psycho-dynamic gradients*, whether viewed from the standpoint of behavior, of neural function or of conscious phenomena.

(2) The repeated use of similar pattern-stimuli must reduce behavior to such a condition that specific interference is no longer present.

(3) Behavior selected under one set of conditions is a mental act different from the same behavior selected under other conditions. We argue for the classification of actions in terms of central determinants, not in terms of ends served.

(4) Automatization is essential in all acts of integration. It begins immediately and proceeds with greater and greater force until the coördination reaches a complete segregation from other systems of behavior. The speed of this tendency may be slow or rapid depending on the complexity of the act selected. *Attention* and *automatization* become the extremes of a transition accomplished through sheer repetition of the same stimulus-pattern.

(5) Resistance and acceptance reactions obey the same law. Avoidance of certain pattern-stimuli may become set off into automatization as well as the acceptance or selection of positive modes of behavior.

(6) There is no fundamental objection to considering attention as a process of association provided we differentiate between initial and subsequent stages. We may indicate, however, that the fact of dominance (clearness) and the facts of the psychodynamic sequence are two separate phases for consideration and should not both be labelled *attention*.

(7) A graphic representation called an 'attention' curve, made on the assumption that efficiency of behavior is a measure of attention, cannot remain level and constitute continuous attention. For example, training a subject in the use of problems to attain a level curve of efficiency is but to substitute automatization for attention. It defeats the purpose of the experiment. A slope in the curve of efficiency is in itself evidence that selection is operating and attention is present.

(8) The selection of a given type of behavior is probably never the result of any one condition but rather the result of composite forces of the entire environmental situation, and no measurement of attention is adequate unless all changes in the

environment are taken into consideration. The importance of this principle has grown more pronounced with the appearance of the *Gestalt* movement.

II. Historical Antecedents

The nervous basis of reciprocal inhibition in the spinal cord was given its final verification by Sherrington.[3] A certain degree of application of the laws of spinal action to principles of cerebral action seems warranted on the basis of the facts of learning and memory. But we must not fail to note that every part of the spinal cord is connected with every other part. Sherrington writes, "Yet we must not lose sight of the physiological solidarity of the action of the group of elements that compose a 'reflex center' in its reflex activity. 'Immediate spinal induction' and the spatial spread of the refractory phase in the scratch-reflexes evoked from separate points show that, intra-spinally, the various component arcs of the type-reflex are connected to something like a unitary mechanism." The fact that events in one part of the nervous system modify the functioning of other parts ought to make the psychologist cautious in accepting any theory of association built on the concept of a single nervous tract. We cannot escape a mass of cross-relationships of one phase of behavior with another, the shifting of which should suggest many a fine problem in psycho-dynamic gradients.

The fact that researches upon attention have a tendency to become complicated with the process of automatization has been known since the earliest experiments by modern laboratory methods. It seems certain that Wundt was aware of this factor as a serious problem prior to 1880. Cattell[4] reported in 1886 that distraction stimuli were more effective on acts which were attended to for the first time. In spite of the fact that Cattell had published the results of his experiments involving the dangers of automatization, it does not seem to have occurred to anyone for a long time that the effect of distraction might not be a constant, but rather a persistent variable which is correlated with the newness of the integration. Therefore most investigators did not see that a temporal function was an important measure of the effect of distraction on attention. Almost all of the early experiments have assumed that, as long as the attended work was kept as variable as possible, all parts of a temporal sequence were equally well attended to. Often these early experiments have missed the possibility that a persistent common form running through the mental work could breed automatization just as the experienced bookkeeper learns to add new columns of old and familiar elements of organization; or just as the accomplished musician can read automatically a complicated new score because that score is cast in a form which makes use of old and familiar stimulus-patterns. It would seem that the failure to control the temporal aspect has been due to the traditional definition of attention as an attribute of dominance, without an adequate interest in the determinants which lead to that dominance.

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[3] C. S. Sherrington, *The Integrative Action of the Nervous System*, 1923, Lect. 4.
[4] J. McK. Cattell, Psychometrische Untersuchungen, *Phil. Stud.*, 3, 1886, 329; The time taken up by mental operations, *Mind*, 11, 1886, 237-242.

In 1892 Swift[5] belittled the dangers of automatization, although acquainted with the work of Wundt's laboratory and Cattell's published work. Swift noted, however, that the effect of distraction diminished if the interference became "monotonous," failing to note anything in common between monotony and automatization. Apparently he seems to have missed the possibility that tendencies of resistance and acceptance are equally capable of automatization. He argued one out of existence but accepted the other. Swift concluded that the degree of attention was inverse to the amount of distraction from auditory and visual stimuli.

Münsterberg[6] tried the effect of introducing voluntary arithmetical work as a distraction to the process of making judgments on auditory intensities and visual distances. He had no check on the extent to which subjects resorted to attentional fluctuation between arithmetical problems and sensory discrimination, and no attempt was made to measure the extent to which the arithmetical operations had been reduced to automatized processes. Hamlin's[7] check on Münsterberg's experiment supported the former of the above criticisms. Her excellent review of research upon attention to that date gives a general indication of the previous failure to see attention as the termination of a gradient process, of selective conditions which operate differently with practice.

It was near the beginning of the present century that Titchener[8] laid the foundation for a definition of attention derived from his limitation of the field of psychology to the study of conscious processes. There is definite evidence in the writings of Titchener that he was interested in the functional descriptions of mind; but the prevailing problems in psychology at the beginning of the present century favored a static definition. Many of the experiments began on the problem of the relation of attention to sensory intensity and the possibility of the control of dominance by preceding mental conditions was often neglected. The procedure used in Münsterberg's experiment (previously cited) is an example of the point at issue. The prevalent acceptance of the terms *foreground* and *background* of attention without a more critical valuation of the evidence for these terms led to the repetition of Münsterberg's experiment by Hamlin. A careful examination of this experiment showed that the problem did not logically concern the relation of background and foreground clearness at all, but was rather a problem of the selective factors for dominance. Hamlin says, "We notice first the evidence upon the nature and effects of the 'distraction.' The subjects usually found that it acted as a spur rather than a check on attention. Their attention was wholly absorbed in the addition for a part of the interval, but as a rule the distractions were not continuous, and there came free instants in which the stimulus flashed into

[5]E. J. Swift, Disturbance of the attention during simple mental processes, this JOURNAL, 5, 1892, 1-19.

[6]Hugo Münsterberg, The intensifying effect of attention, this JOURNAL, 1, 1894, 39-44.

[7]A. J. Hamlin, Attention and distraction, this JOURNAL, 8, 1896, 3-66.

[8]E. B. Titchener, Postulates of a structural psychology, *Philos. Rev.*, 7, 1898, 449-465.

consciousness with great clearness and distinctness." How many times have the reports on background clearness been due to these "free instants," *i.e.* momentary fluctuations!

Certainly there is little evidence here for a description of background in the usual sense. We must note that the incentives to attend, as a condition of the selective process, are altered the moment the distraction is used. If our criterion holds true that behavior under one set of selective conditions is a different mental act from the same behavior under other selective conditions, then it seems that we have an adequate explanation of Münsterberg's and Hamlin's results, not in terms of the effect of attention on sensory intensity, but rather in terms of the differences in effectiveness of the conditions of attention.

Most of these experimenters averaged together all reactions, whether they came in the beginning, the middle, or at the end of the experiment, failing to take into account the possibility of a change in the nature of the mental process as the individual did the same kind of work over and over again. Have we here the results of automatized acts averaged in with the results of volitional or attended acts without differentiation? Some weak attempts were made to control automatization. The initial 'practice curve' has long been recognized. But even in this control there has been the persistent uncritical assumption that after a subject has passed through the period of practice he is still exhibiting acts of attention. It seems astonishing that so many experimenters have given no indications of the possibility that the practice curve itself may be the only part of the behavior that is really to be called attention, a persistent gradient always found in the initial stages of newly integrated behavior.

Some of the early experiments are interesting examples of the above lack of control. In 1896 Birch,[9] working in the Cornell laboratory, used odors as a method of distracting attention given to the discrimination of pairs of sounds. The experiment was partially lacking in the control of the temporal factor, but there are one or two outstanding results which appear to be important in the development of our theory of attention. Birch deplored the fact that she had not given a great amount of time in the training of her subjects for the mental work which was to constitute a measure of attention. She found that the curve of attention constantly seemed to fall during the intervals of no distraction. As the subjects were given more and more periods of work there seemed to be a tendency for the number of correct judgments to increase under the effects of distraction. She was afraid that this was due to lack of previous training; but there is a possibility that this would always occur under continued distraction and is but an indication of automatized avoidance of the disturbing stimulus.

Darlington and Talbot,[10] in the same laboratory, attempted to show the effect of short musical phrases as a distraction. Their general conclusion was that discrimination was better during distraction than without distraction.

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[9]L. G. Birch, A study of certain methods of distracting the attention, this JOURNAL, 9, 1897, 45-55.

[10]L. Darlington and E. B. Talbot, A study of certain methods of distracting the attention, this JOURNAL, 9, 1898, 332-345.

But again they gave no curves to show how rapidly this capacity to work better under distraction developed, whether it was abrupt and constant, or gradual and accumulative. All we can conclude from this experiment is that some conditions for selection are better than others. Fear that one is going to be bothered is apparently an indication of a special selective condition and may be accompanied by a pronounced difference in efficiency of behavior. The neglect of the temporal attribute seems to be a persistent tendency among investigators holding a theory of attention as a state. Titchener's[11] formulation of attention in terms of attributive clearness was followed by experiments carrying his basic assumptions. The year after the publication of Titchener's book Geissler[12] brought forward an experiment on the relation of attributive clearness to efficiency of behavior, based on visual processes. Four years later the work was carried forward in a similar way by Dallenbach[13] who attempted to correlate attributive clearness of auditory states with efficiency of behavior. He gave a correlation (product-moment method) of .85 as the lowest result when comparing efficiency of work performed with verbal reports on clearness.

Dallenbach says, "Previous experimenters have found that distractors very soon lose their power of compelling the attention; the observers become habituated." Here we have a direct allusion to the fact that resistance to a distraction can become automatized. Here also we seem to see, for the first time, a real attempt to control the problem, for Dallenbach tried to keep the distractions constantly varied in order that 'habituation' might be kept at a minimum. Whether he succeeded or not could only have been told by some sort of a temporal curve, showing the changes in efficiency as the various distractions continued. In all probability, even a variable distraction can ultimately be resisted. But it is a very significant fact that Dallenbach reversed the opinions of previous investigators in the statement that the distractions impaired efficiency instead of increasing it. It would seem that this conclusion would not have been possible had he not been very careful in controlling the fact of 'habituation.' We need not quibble concerning Dallenbach's method of securing evidence of attention. Morgan's substitution of a direct measurement of overt behavior for the verbal report or 'introspective report' has nothing to do with the principle. Since the correlation between the two types of behavior is high we may regard either as an adequate measurement of attention. The fact is, we seem to have here evidence that Dallenbach measured the *initial* effects of distraction, while previous experimenters had been measuring *cumulative* effects and securing, therefore, opposite and contrasting results.

It is evident that so far there were no attempts to show, by means of a smooth curve, how distractions can affect efficiency of behavior as time passes. Bits of incidental evidence occasionally were given, but not with the motive

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[11]E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, Lect. i, v, and vii.

[12]L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 473-529.

[13]K. M. Dallenbach, The measurement of attention, this JOURNAL, 24, 1913, 465-507. Cf. also The measurement of attention in the field of cutaneous sensation, *ibid.*, 27, 1916, 443-460.

of showing the process nature of the resistance to distraction. Indications of the principle may be noted in some of the studies on the curve of efficiency for the acquisition of skill. Batson[14] noted that a distraction was more effective in the beginning than toward the end of the acquisition of a skilled performance. He held that the effect of a distraction is never totally lost, a statement that seems to indicate that we are dealing only with an approach to limits—a psychological application of the notion of relativity. Some of the experiments on reaction-time under the influence of distraction remind one of the days of the older Wundtian laboratory and the warning of Cattell on the importance of automatization. The contribution of Evans[15] on the relation of distraction to reaction-time is an example. Evans used light, sound, and touch distractions in simple reaction-time experiments and obtained temporal curves which indicated a definite gradient. The repetition of a simple reaction implies a fairly rapid automatization rate and would not be regarded by all as a good example of the effect of distraction on attention. As a matter of fact, we see no reason why Evans' curves may not be regarded as a measure of the attention-automatization gradient.

Dissatisfied with Dallenbach's measurement of attention by introducing verbal reports on attributive clearness, Morgan[16] decided to carry forward a distraction experiment in which all computations should be based on immediate and direct behavioristic measurements. Because he apparently felt that there was no safe criterion as to what constituted attention in terms of behavior, Morgan seems to have decided definitely to avoid the word *attention*, allowing his readers to make whatever inferences were desired as to the principles of mental control behind behavior. His experiment is generally classified as one on attention, from its apparent resemblance to traditional experiments.

Morgan found that the average reaction-time for the solution of difficult problems is less under noise than under quiet conditions. He also found that the first problems in a series under distraction require a longer time for solution than later ones. The increase in speed for the later problems more than compensated for the slowness in working the initial problems of the series. His method of averaging together ten or more problems concealed the possibility of a smooth curve of distraction. The fact that he did not average the entire period together was the saving point in his technique of computation; it left something for the process nature of adaptation to distraction. He measured the concomitant motor effects by means of key pressure and found that the increase of efficiency was accompanied by a dynamogenetic effect of distraction. Although Morgan's statement that distraction increased efficiency as measured by speed, we must remember that this statement is the result of averaging together several problems in the same distraction period and the statement means just what the older statements meant, *i.e.* the cumulative effects of distraction, or automatized resistance to distraction accompanied by increased incentives to work, not the initial effects of distraction on attention as measured by

[14]W. H. Batson, Acquisition of skill, *Psychol. Monog.*, 21, 1916, (no. 91), 86.
[15]J. E. Evans, The effect of distraction on reaction-time, *Arch. Psychol.*, 37, 1916.
[16]Morgan, *op. cit.*

Dallenbach. The fact that Morgan did take into account the difference between initial and cumulative effects of distraction and mentioned the difference is the saving point in his work which suggests the possibility of a point of view leading to a definition of attention and automatization as extremes of a gradient process. Morgan used noises for distraction, which were unique in that they were unusually severe. The same type of noise was used throughout one sitting.

The precision of Morgan's experiment places it in a position of importance in solving the problems relating to the nature of attention. Direct experiments on the problem have not been attempted in the European laboratories; but there are some researches which seem to contribute interesting possibilities of explanation. Claparède,[17] for instance, had begun an experiment on fatigue which resolved itself later into an experiment on attention. When a subject was given the task of contracting both hands simultaneously on a pair of ergographs until paralysis resulted, the cessation of *trying* to contract one hand was accompanied by a resumption of actual movement of the other hand although no rest-pause had been given. Now the sudden resumption of movement by a member that was previously paralyzed with fatigue seems to offer something important in the nature of the central controls of volition. Claparède suggested an explanation in terms of the drainage of energy from central reservoirs which had been divided while innervating two hands, but which are later allowed to throw their entire force into one hand. In terms of behavior, we know that there was present an interference of efficiency when two hands were used, and it would seem that the postulation of reservoirs carries us beyond scientific necessity.

An extension of Claparède's work was accomplished by one of his pupils, Rimathé,[18] using a similar ergograph, but so arranged that homolateral pairs of muscles could be used. He found that the same principles held for muscles on the same side of the body, an indication that the phenomenon could not be correlated with the paired functions of the cerebral hemispheres.

The fact seems to be, however, that we have here what is essentially a distraction experiment. Verbal reports on the part of subjects in such fatigue experiments indicate that the process of producing fatigue-paralysis is accompanied by exceedingly uncomfortable feeling from the members being used and that there were changes in incentives when the fatigue pains were removed from one of the hands—enough to account for the increase of efficiency. While Morgan did not carry his experiments far enough to produce a great amount of sensible fatigue, we may indicate here that in all probability we must be on our guard against the inhibiting effects of proprioceptive as well as exteroceptive stimuli, and regard them as possibilities of distraction.

III. Apparatus and Procedure

With the foregoing criticisms in mind, we wished to show the changes in efficiency for each successive problem carried on with-

[17] E. Claparède, L'ergographie bilatérale, *Arch. des sci. phys. et nat.*, 44, 1917, 71.

[18] F. Rimathé, Contribution à l'étude de l'ergographie bilatérale et simultanée, *Arch. de psychol.*, 19, 1924, 128-162.

out disrupting the temporal order, both with and without distraction. At the same time we wished to make the period of work short enough to reduce the chance of extreme automatization of the problems. It will be seen that, instead of eliminating the practice curve, we have deliberately kept our observations within its scope. Efficiency of behavior will be measured in terms of immediate and direct performance, with enough verbal report allowed to throw more light on the causes of irregularities. In order that the cumulative effect of distraction might be taken into account we decided to average together all first problems apart from the average of all second problems of the series, and these in turn apart from all third problems, and so on. Our graphs will represent the function of reaction-time plotted against position in the series.

In spite of the fact that Morgan's problems were much better than those used in many other experiments, in that they were much more difficult and therefore less rapidly automatized, we believed that it would be possible to frame problems which were equally difficult and which at the same time placed a penalty on any fluctuations of attention from the work at hand. Any deviation from the following problems will endanger forgetting the sum which *S* is carrying and produce a corresponding long reaction brought about by the necessity of going back to the beginning of a problem, or there may be a false answer.

Ordinarily arithmetical operations may be automatized as generalized pattern-situations; so if they are to be used they must be placed in unusual combinations or environments. We framed 60 problems involving the selection of single digit numbers from a row of mixed letters and numerals, *S* being told to add these numbers as he discovered them and to write their sum on a strip of paper. The following is an example of one of the problems:

Gd7Fc8NfEaW9MBcOPT5FzA4NvCXz6MkhgP9xtb.

The numerals were scattered throughout the series of symbols in no predictable order either as to spacing or kind. The symbols were secured by chance as was also the spacing of the various digits.

The accumulation of muscular tonus during the distraction was an important point in Morgan's work and was made an index to the intensity of the mental process. We had no objection to Morgan's method of denoting the presence of the factor and had no doubts concerning his statements of the results. Because our *S*s wrote their answers on paper instead of pressing keys, we used writing pressure instead of key pressure, indicating the amount by means of a tambour recorder on a strip of kymographic paper. The same pencil was used throughout all runs of the experiment in order that there might not be variations of behavior due to differences in hardness of the writing point.

Morgan's distractions, as we have indicated, were more than usually severe and we agreed on the whole with his selection. We had but one difficulty in selecting the noise to be used, the matter of the difference between monotonous or constant noises and variable noises. If we used the evidence based on a comparison of Morgan's and Dallenbach's results, the former would produce rapid and the latter slower automatization. We did not think it necessary to use the great variety of noises introduced by Morgan, since many of them must have given identical results.

Our choice finally fell on two methods of creating a noise distraction, a Klaxon automobile horn placed about two feet from *S*'s ear, and a phonograph operated with a loud needle, the amplifying horn being adjusted as closely as possible to *S*'s head. A monologue in a male voice delivered its supposedly humorous story into the ear of *S* while he was trying to solve the problems outlined above. In the preliminary experiment the phonograph was used entirely and in the later experiments we used the automobile horn.

The room in which *S* sat was illuminated entirely by artificial light, the walls being painted black and free from all light-distracting objects. The experiment was conducted in a section of the building which was relatively free from extraneous and uncontrolled noises. A 60-w. lamp overhead gave a small amount of illumination all the time, while another 60-w. lamp on the table at *S*'s side was so screened that it threw a bright light on the card of problems held in front by a bracket. The table lamp was so connected that *E* could, without entering the room, throw it on as a signal to begin work and turn it off as a signal to stop. *S* was always alone in the room while doing the problems; the kymograph, recording instruments, motor and control switches were all in another room, sound-insulated from him.

The apparatus on *S*'s side of the partition consisted of the following items. An endless belt, for exposing the code numbers indicating which problem was to be solved, was arranged in such a manner that the pushing of a lever turned a new code-stimulus into view and at the same time drew a fresh space on the strip of white paper used for recording the answers. The strip of paper on which the answers were written was drawn on a spindle so as to run over a large tambour on which very stiff rubber had been stretched and then covered with a wooden plaque. The tambour thus constituted an indicator of writing pressure. The amount of deflection of the tambour was imperceptible to the *S*s, only four of whom knew that the writing-pressure was being recorded. A tube extended through the wall from the writing-pressure board to the tambour-recorder on the kymograph. The tension of the writ ng-pressure board was calibrated by placing weights on the paper and noting the amount of deflection of the recording needle.

A Lombard pneumograph was strapped by means of a broad leather band to the upper portion of the thorax and was used for the same purpose as that used by Morgan, *i.e.* to secure indications of articulation. Since blood circulation was also being measured the respiratory changes constituted one of the checks on vaso-motor activity. The left fore-finger and second finger were pressed firmly to a rubber bulb and fastened to it by means of adhesive tape. A tube led from the bulb to the piston-recorder, the usual arrangement for the

Lombard-Pillsbury plethysmograph. The arm of *S* was now suspended in a sling hung from the ceiling in order to allow for body-movements without interfering with the operation of the plethysmograph. It was hoped that this portion of the apparatus might produce evidence in addition to Morgan's statements on articulation and key-pressure.

The kymograph used was developed in our laboratory.[19] It employed high tension discharge of electric sparks as a means of making a graphic record on white paper. The graphic record consisted of the following lines. (1) The perforated line of blood-volume taken with the piston-recorder. (2) The time-line (in sec.) taken with period perforations controlled by connecting the spark discharge in series with a mercury-contact seconds-clock. Some of the records were taken with the kymograph being driven by a synchronous motor operating on 60-cycles current from the power-house. Checks were made frequently for variations of speed, but as the variability of the motor was less than 1% of the variability of the data, there seemed to be no reason for rejecting the method of measuring time in terms of relative linear distances on the kymograph record. (3) A perforated line from the needle of a tambour giving a record of breathing changes. (4) A perforated line controlled by *E*'s switch indicating the exact moments the distraction was turned on and off. 5) An ink-needle tambour giving a record of the amount of writing-pressure exerted by *S*, and also a means of ascertaining the writing-time which was not counted with the total reaction-time.

At the beginning of the first sitting, *E* told *S* the nature of the problems and worked one out, in order that the procedure might be perfectly clear. The noise was turned on for a moment in order that *S* might become acquainted with the nature of the distraction. This appeared necessary, especially with the automobile horn, because of the tendency of the *S*s to jump from their chairs if the distraction were turned on without previous warning. *S* was told then to start working just as soon as the lamp on the table lighted, to work as rapidly as he could, to remember that both speed and accuracy records were being taken, and to permit no interference from anything until the light on the table was extinguished.

S worked upon not more than 8 of the problems, when the distraction was turned on at exactly the beginning of a problem. Thus the preliminary quiet period was, on an average, about a minute-and-a-quarter long. The distraction remained on for exactly 6 problems, then it was abruptly discontinued. Then there followed 6 or more problems worked in absolute quiet. The 6 problems before the distraction, the 6 during the distraction and the 6 after the distraction were measured and these measurements given a place in frequency curves, a curve being made for each position in the series. Although there were often as many as 8 problems before the distraction, only the 6 preceding the distraction were tabulated. We varied the number of problems in the first and third periods in order to prevent anticipation of the end of the period by the *S*s. In the first period this prevented fear of the distraction affecting the final problem, and in the third prevented the possibility of the 'end spurt.' It

[19]Adelbert Ford, Recording apparatus: the electrokymograph, *J. Exper. Psychol.*, 7, 1924, 157-165.

is true that the working period in our experiment was much shorter than in Morgan's. Some of the *Ss* complained that they had scarcely got 'warmed up' to the work when they were forced to stop. But it must be remembered that we were working under the assumption of the possibility that excessive training removes attention, that the best place for measurement of attention is in the initial stages of the practice curve.

After each sitting *S* was asked to describe what happened while he worked. Since the results of the experiment were to depend heavily on the efficiency of performance, this 'introspection' was introduced merely to secure evidence concerning any unpredictable conditions. After they had finished their voluntary description they were asked a few direct questions: "Did you have any emotion?" "Did you lose your place while working?" "What happened when the noise stopped?" "Have you ever had experience in an occupation which required a great amount of arithmetical addition?" "What effect do you think the noise had on your speed and efficiency?" "Did you have any difficulty with the manipulation of the apparatus?" They were also asked to make statements concerning health, age, state of rest or fatigue, and mood.

During the first run there were probably other distractive conditions than the noise itself. The *Ss* were not accustomed to the conditions of the psychological laboratory. Many were more or less nervous upon entering and being seated in an eerie, black room, knowing that "something in their mind was being measured." The experience of being strapped into various instruments was another source of novel stimulus which was certain to bring about changed attitudes toward the work at hand. But there was strong evidence that these uncontrolled environmental and attitudinal effects rapidly disappeared. Several of the *Ss*, forgetting that they had a bulb in their fingers and a pneumograph on their chests, walked off as soon as the experiment ended, tearing the instruments from their respective tubes. The nervousness from the fear of being "mentally measured" likewise disappeared after the first run and they reported taking the experiment as a matter of course. All but three of the *Ss* were undergraduate students in elementary psychology. We have since regretted that we did not give every *S*, before the principal experiment, a period of special work to produce environmental orientation. From the point of view we have taken here, this would mean "automatization to to uncontrolled distractors." In all probability our *Ss* did not get accustomed to the laboratory environment until the second sitting of the experiment.

It may be said that the introduction of an automobile horn as a distraction was likely to produce emotional effects which constituted an unmeasured variable. This criticism cannot be answered in any form of distraction experiment. There are always emotional and feeling effects and they cannot be totally eliminated. It may even be possible that the situation which brings about new integrations necessitating strong dominance, *i.e.* attention, involves the very conditions which make emotions unavoidable. We are not at all certain that in all situations where one pattern opposes another the emotion is not the working principle of the distracting effect.

IV. The Preliminary Experiment

The first runs in the experiment were started in the fall of 1921 and were made largely with a view toward perfecting the apparatus. Apparently the phonograph did not give nearly as much of an emotional shock as did the automobile horn. While we had thought that the phonograph might constitute a variable distraction, and so prevent automatization, we found that most of the *Ss* reported that they heard nothing from the instrument that was more than vaguely meaningful, there being only an occasional awareness, confused and fragmentary, of a male voice talking in strident tones. This seemed to have reduced the effectiveness of the phonograph to that of a monotonous distractor, much inferior to the horn. In the later experiments we abandoned the phonograph.

TABLE I

Preliminary Experiment

Measurements of time (sec.) required for the solution of problems. An experimental period consisted of 18 consecutive problems: 6 under quiet conditions, 6 under distraction (phonograph), and 6 under quiet conditions again. Based on frequency curves of 68 experimental periods

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.	Errors
11.847	14.733	16.176	4.794	11.382	20.970	11
12.176	15.000	16.412	4.457	11.955	20.869	6
13.494	15.400	16.353	4.467	11.886	20.820	7
13.851	15.833	16.824	4.650	12.174	21.474	15
13.193	15.429	16.412	4.747	11.665	21.159	14
13.294	16.000	17.353	4.492	12.861	21.845	11
16.273	18.091	19.000	5.149	13.851	24.149	11
14.352	16.000	16.824	4.269	12.555	21.093	12
19.811	18.077	17.210	3.641	13.569	20.851	14
14.381	16.833	18.059	4.704	13.355	22.763	10
15.811	17.545	18.412	4.808	13.604	23.220	6
13.823	16.333	17.588	4.375	13.213	21.963	16
15.624	16.600	17.088	3.845	13.243	20.933	16
15.014	15.750	16.118	3.659	12.459	19.777	12
13.880	16.058	17.147	3.784	13.363	20.931	7
14.732	16.636	17.588	3.975	13.613	21.563	15
15.051	16.429	17.118	4.014	13.104	21.132	12
12.737	15.363	16.676	3.973	12.703	20.649	10

The figures in Table I give the results of 68 experimental runs, an average of four from each person, in terms of the computed mode, median, mean, and mean deviation, and the number of errors committed out of 68 problems, the phonograph being used as a distraction. We used the mean as a measurement of the central tendency throughout all work. The median and com-

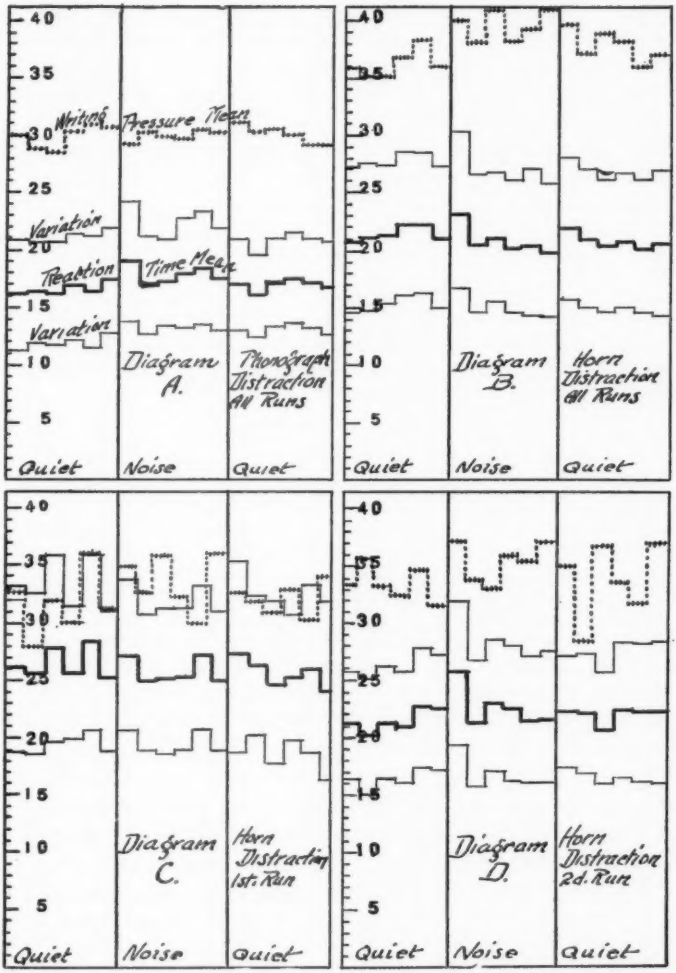
40
35
30
25
20
15
10
5
Writing Pressure Mean
Variation
Reaction Time Mean
Variation
Quiet
Noise
Quiet
Diagram A.
Phonograph Distraction All Runs
Diagram B.
Horn Distraction All Runs
Diagram C.
Horn Distraction 1st. Run
Diagram D.
Horn Distraction 2d. Run

FIG. 1

puted mode were given for reasons to be considered later. Diagram A (Fig. 1) shows the facts of the table in graphic form wherein the vertical coördinate represents reaction-time and the horizontal coördinate represents the series of problems worked. The levelness of the reaction-time measurements for the first 6 problems in all the graphs rather gives one the notion that the slope of the curve of practice is very gradual. The values in all tables for every problem in the series are based on averages of 38 runs or more. The headings of the tables give the number of measurements averaged together.

The following points are to be noted in the curve of means for Table I, based on 68 runs, with the phonograph as a distraction:

(1) The practice effect is not present.

(2) The initial problem in the distraction period displays a conspicuously greater reaction-time than any other in the series. The entire 6 problems of the distraction period are longer than those of the quiet periods.

(3) There is no evidence of an initial long reaction-time for the last quiet period.

(4) There is a very poor correlation between writing-pressure and the reaction-time.

V. Methods of Computation

We have given the mean deviation in our tables for the purpose of indicating any changes in the variability of efficiency under various conditions. The fine lines on the diagrams show the limits of mean deviation on each side of the mean. The causes of deviation are important for consideration, although to some extent problematical. We should probably have to consider differences in *ability* in this type of arithmetical work, differences in the *difficulty* of the problems being used, changes in the *attitude* of the subjects from problem to problem, *fatigue* effects, *minor distractions* from discomfort and other organic conditions, as well as other factors impossible to identify.

Practically all of the frequency curves secured for every problem in the series show evidence of skewness. The measurement for the skewness of a curve was suggested by Pearson, and we have given enough of the curve measurements to enable computation of this factor. Pearson had noted that in most skewed curves the distance of the mode from the mean was three times that of the distance of the median from the mean. This gave him a means of computing the mode, given the mean and the median, with but small error from the position

of the observational mode. Our use of the computed mode in our tables is presented solely as a means of denoting the relative amount of skewness. Pearson's suggestion for a formula for skewness which would not be expressed in original units was

$$\text{Skewness} = \frac{\text{Mean} - 3(\text{Mean} - \text{Median})}{\text{Standard Deviation}},$$

but there is no essential reason for abandoning the original units as long as we make comparisons in homogeneous data. Therefore it seems that the difference between the mean and the median may be taken in our tables as an indication of the skewness.

The meaning of skewness must affect our interpretation of results. Whenever the mean shows a definite change, either higher or lower, we must know whether the change was caused by a few extreme and exceptional cases or was brought about by a tendency for a small amount of change in all cases. If the distraction, for instance, should cause an *S* to forget his accumulated sum, he might occasionally have to go back and pick up his work from the beginning of that particular problem. This might not occur often; but such a condition would have a tendency to add cases on the end of the curve already having the broader range and increasing the skewness more than ever. On the other hand, if the distraction has the effect of destroying automatization and *S* is merely displaying a slower associative tendency, we might expect all the reactions to be longer by a fairly constant increment. It is evident that the increase in skewness would be accompanied by a small increase in the mean deviation. If the distraction had the greater effect on the shorter reactions, *i.e.* the ones which were successfully completed without the subject losing his place, then the mode would be thrown farther away from the negative end of the distribution and the amount of skewness would be seen to decrease.

The amount of skewness in Table I decreases the moment *S* begins to work under distraction. The mode is thrown nearer the positive end of the distribution, an indication that the effect of the distraction is probably general on all reactions and not just a pronounced effect of a few reactions which are the result of *S* losing his sum and having to go back to the beginning of the problem.

Although our *S*s showed a greater tendency to commit errors than did Morgan's, there seems to be no indication of a correlation between number of errors and the presence of distraction. It is true that there are more errors in the first half of the distraction-period than in the last half; but in view of the great variability of errors from time to time it seems that we cannot be sure that this is not coincidence.

In the experiments where we used the phonograph as a distractor there seemed to be very little evidence of any correlation between the writing-pressure used by the *S*s and the length of

time for working out the problems, and no observable transition in writing-pressure at the time the distraction appeared. We are inclined to believe that this failure to get results was due to the weakness of the distraction used.

Reports by the *Ss* seemed to give some clues as to mental attitude.

Most *Ss* reported annoyance at the beginning of the noise-period and relief at the beginning of the following quiet-period. Nearly all were able to state that they were retarded at the beginning of the noise-period, but only 4 that they recovered speed as the noise-period progressed. None of the *Ss* were able to tell the story given in the monologue even after the fourth repetition of the same record. One *S*, a graduate student in psychology, reported after the first run that she had not heard the record at all and did not know that the phonograph had been turned on. She apparently received no effect at all from the distraction even by measurement of the reaction-times. No other *S* ever reported complete obliteration of the distraction. Five of the 17 *Ss* in this first group reported that they customarily studied with a phonograph playing in their rooms, a temporary fad among students in the University at that time. The composite curve of these 5 *Ss* showed such great variability that we cannot state the influence of the previous training.

VI. Later Experiments

Whereas in the preliminary experiment with the phonographic distraction we had used 16 *Ss*, in the fall of 1922 we arranged to use 41, 38 undergraduates and 3 graduates in psychology. We corrected the apparatus in such a manner as to simplify *S*'s work in changing problem-cues, and to have a finer calibration to the writing-pressure board. The same problem card was used as before. We moved the spark commutator into a distant room to prevent the least possible noise, and used a quiet synchronous motor for moving the kymographic mechanism, measuring time by relative distances on the kymograph-paper. We substituted the automobile horn for the phonograph.

Table II gives the results of 183 sittings from 41 *Ss*. Diagram B (Fig. 1) shows the results in graphic form. Table VI shows the measurements of writing pressure in these same sittings. Table III is a composite result of first sittings for 38 *Ss*, the first run only. Diagram C (Fig 1) gives the graphic illustration. Table VII gives the writing-pressure measurements for Table III. Table IV and Diagram D (Fig. 1) give the results of the second consecutive sitting for the same 38 *Ss* and Table VIII gives the corresponding

TABLE II

TIME MEASUREMENTS

From frequency curves of 183 experimental periods

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.	Errors
15.621	19.167	20.940	6.259	14.681	27.199	47
13.463	18.579	21.137	6.425	14.712	27.562	41
16.751	19.857	21.410	6.036	15.374	27.446	34
13.359	19.273	22.230	6.179	16.051	28.409	34
18.061	20.833	22.219	6.150	16.069	28.369	40
17.253	19.769	21.027	6.123	14.904	27.150	34
18.947	21.667	23.027	6.250	16.777	30.277	34
15.741	18.923	20.514	5.899	14.615	26.413	52
16.659	19.571	21.027	5.613	15.414	26.640	45
17.152	19.320	20.404	5.669	14.735	26.073	30
15.983	19.091	20.645	6.235	14.410	26.880	31
13.067	17.667	19.967	5.737	14.230	25.704	45
18.402	20.684	21.825	6.211	15.614	28.036	43
16.339	19.421	20.962	6.016	14.946	26.978	40
15.712	18.840	20.404	5.704	14.700	26.108	41
15.249	18.933	20.775	5.856	14.919	26.631	42
18.249	19.555	20.208	5.788	14.420	25.996	35
15.103	18.739	20.557	6.291	14.266	26.848	32

TABLE III

TIME MEASUREMENTS

The work of 38 *Ss* without previous training

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.	Errors
27.181	26.429	26.053	7.216	18.837	33.269	8
22.642	24.600	25.579	6.820	18.759	32.399	4
20.315	25.333	27.842	8.141	19.701	35.983	5
23.842	25.000	25.579	5.684	19.895	31.263	11
26.078	27.500	28.211	7.612	20.599	35.823	13
23.484	24.600	25.158	5.745	19.413	30.903	6
22.998	25.666	27.000	6.684	20.316	33.684	9
20.704	23.500	24.895	5.983	18.912	30.878	11
22.706	24.200	24.947	6.310	18.637	31.257	11
21.790	24.000	25.105	6.111	18.994	31.216	8
27.000	27.000	27.000	6.263	20.737	33.263	7
25.210	25.000	24.895	5.947	18.948	30.842	12
20.898	25.000	27.052	8.291	18.761	35.343	11
25.686	26.000	26.157	6.174	19.983	32.331	13
25.422	25.000	24.789	7.105	17.684	31.894	11
23.484	24.600	25.158	5.640	19.518	30.798	12
19.314	26.666	25.842	7.291	18.551	33.133	9
21.106	23.000	23.947	7.896	16.051	31.843	8

TABLE IV

Time Measurements

The work of 38 *Ss* one week after the first run tabulated in Table III

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.	Errors
18.015	20.111	21.159	4.455	16.704	25.614	10
18.794	19.666	20.102	5.064	15.038	25.166	12
22.629	21.859	21.474	4.728	16.746	26.202	4
21.106	21.000	20.947	4.947	16.000	25.894	4
16.986	20.750	22.632	5.152	17.480	27.784	6
19.868	21.500	22.316	4.997	17.319	27.313	8
17.842	23.000	25.579	6.324	19.255	31.903	4
16.394	19.500	21.053	5.535	15.518	26.588	12
19.776	21.750	22.737	5.773	16.964	28.510	14
14.800	19.600	22.000	5.895	16.105	27.895	1
18.893	20.333	21.053	5.058	15.995	26.111	5
18.761	20.429	21.263	5.186	16.077	26.449	9
19.974	21.500	22.263	4.936	17.327	27.199	9
20.998	21.666	22.000	5.263	16.737	27.263	7
17.946	19.666	20.526	4.792	15.734	25.318	10
18.474	21.000	22.263	5.883	16.380	28.146	5
14.394	19.500	22.053	5.961	16.092	28.014	10
18.894	21.000	22.053	6.072	15.981	28.125	5

TABLE V

Preliminary Experiment

Measurement of writing pressure while writing the answers to problems. An experimental period consisted of 18 consecutive problems: 6 under quiet conditions, 6 under distraction (phonograph) and 6 under quiet conditions again. Based on frequency curves of 68 experimental periods

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.
9.840	10.000	10.080	2.341	7.739	12.421
8.871	9.375	9.627	2.340	7.287	11.967
10.071	9.735	9.567	2.250	7.317	11.817
10.092	10.100	10.104	2.280	7.824	12.384
7.601	9.409	10.313	2.650	7.663	12.963
8.172	9.500	10.164	2.700	7.464	12.864
9.906	9.750	9.672	2.562	7.110	12.234
8.898	9.722	10.134	2.489	7.645	12.623
11.305	10.375	9.910	2.289	7.621	12.199
10.508	10.100	9.896	2.393	7.503	12.289
10.041	10.083	10.104	2.304	7.800	12.408
9.381	9.863	10.104	2.453	7.651	12.557
11.331	10.643	10.299	2.972	7.327	13.271
10.123	10.071	10.045	2.737	7.308	12.782
8.580	9.666	10.209	2.864	7.345	13.073
10.063	10.071	10.075	2.618	7.457	12.693
9.008	9.500	9.746	2.556	7.190	12.302
9.527	9.643	9.701	2.581	7.120	12.282

TABLE VI

WRITING-PRESSURE MEASUREMENTS

The following values are based on frequency curves of 183 experimental periods

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.
5.849	7.921	8.957	3.614	5.343	12.571
6.575	7.974	8.768	3.286	5.482	12.054
6.111	7.853	8.724	3.424	5.300	12.148
6.412	8.310	9.259	3.622	5.637	12.881
6.604	8.630	9.643	3.934	5.709	13.577
5.927	8.033	9.086	3.757	5.329	12.843
6.881	9.011	10.076	4.029	6.047	14.105
7.295	8.763	9.497	3.848	5.649	13.345
7.909	9.447	10.216	3.871	6.345	14.087
7.559	8.909	9.584	3.942	5.642	13.526
7.418	9.024	9.827	3.788	6.039	13.615
7.190	9.312	10.373	3.984	6.389	14.357
7.893	9.283	9.978	3.555	6.423	13.533
7.333	8.563	9.178	3.754	5.424	12.932
7.992	9.100	9.654	3.543	6.111	13.197
7.222	8.786	9.568	3.690	5.878	13.258
6.835	8.329	9.076	3.520	5.556	12.596
6.796	8.442	9.265	3.750	5.515	13.015

TABLE VII

WRITING-PRESSURE MEASUREMENTS

The following values are taken from the work of 38 *Ss* without previous training

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.
5.217	7.125	8.079	2.472	5.607	10.551
4.816	6.500	7.342	2.801	4.541	10.143
6.552	7.500	7.974	2.079	5.895	10.053
5.948	7.000	7.526	2.213	5.313	9.739
5.948	8.000	9.026	3.561	5.465	12.587
6.268	7.300	7.816	2.860	4.956	10.676
8.078	8.500	8.711	2.711	6.000	11.422
8.036	8.100	8.132	2.514	5.618	10.646
9.458	9.100	8.921	2.249	5.883	10.381
9.611	8.625	8.132	2.388	5.744	10.520
10.552	8.500	7.474	2.210	5.264	9.684
8.115	8.091	9.079	2.091	5.988	10.170
7.226	7.900	8.237	2.643	5.594	10.880
6.658	7.500	7.921	2.500	5.421	10.421
7.132	7.500	7.684	2.368	5.316	10.052
7.422	8.000	8.289	2.305	5.984	10.594
7.394	7.500	7.553	2.132	5.421	9.685
9.410	8.750	8.420	2.450	5.790	10.870

TABLE VIII

Writing-Pressure Measurements

The following values are taken from the work of 38 *Ss* one week after the first run tabulated in Table VIII

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.
4.423	7.071	8.395	3.499	4.896	11.894
5.208	7.666	8.895	3.446	5.449	12.341
6.763	7.833	8.368	3.091	5.277	11.459
5.288	7.166	8.105	2.402	5.703	10.507
7.328	8.250	8.711	3.169	5.542	11.880
6.606	7.500	7.947	2.789	5.158	10.736
7.314	8.666	9.342	3.537	6.805	12.879
5.342	7.500	8.579	2.828	5.751	11.407
7.106	8.000	8.447	2.997	5.450	11.444
5.447	7.833	9.026	3.666	5.360	12.692
7.606	8.500	8.947	3.474	5.473	12.421
4.468	7.700	9.316	3.715	5.601	13.031
7.322	8.300	8.789	3.094	5.695	11.883
6.342	6.900	7.179	2.660	4.519	9.839
8.611	9.100	9.263	3.515	5.748	12.778
4.694	7.214	8.474	3.388	5.086	11.862
4.258	6.700	7.921	2.957	4.964	10.878
4.500	6.500	9.316	5.256	4.060	14.572

TABLE IX

Time Measurements

The following values are taken from the work of 38 *Ss* two weeks after the first run tabulated in Table III and one week after the second run tabulated in Table IV

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.	Errors
16.626	18.454	19.368	4.637	14.731	24.005	7
15.448	16.500	17.026	4.738	12.288	21.764	13
16.965	18.111	18.684	3.512	15.172	22.196	9
12.368	17.000	19.316	5.277	14.039	24.593	7
21.842	21.000	20.579	4.368	16.211	24.947	4
13.684	17.000	18.658	5.296	13.362	23.954	8
20.448	20.500	20.526	5.291	15.235	25.817	9
16.000	18.000	19.000	4.737	14.263	23.737	12
16.684	19.000	20.158	5.244	14.914	25.402	5
17.336	18.200	18.632	4.067	14.565	22.699	9
14.842	18.000	19.579	4.407	15.172	23.986	5
14.368	17.000	18.316	4.715	13.601	23.031	11
20.684	21.000	21.158	5.684	15.474	26.842	9
17.790	18.000	18.105	4.280	13.825	22.385	5
18.052	19.000	19.474	5.000	14.474	24.474	5
15.294	17.800	19.053	4.798	14.255	23.851	11
17.106	19.000	19.947	4.789	15.158	24.736	7
17.682	18.666	19.158	4.737	14.421	23.895	5

TABLE X

WRITING-PRESSURE MEASUREMENTS

The following values are taken from the work of 38 *Ss* two weeks after the first run tabulated in Table VII and one week after the second run tabulated in Table VIII

Mode	Median	Mean	M.D.	M.−M.D.	M.+M.D.
6.175	7.725	8.500	3.765	4.737	12.263
5.184	7.500	8.658	3.235	5.423	11.893
4.937	7.277	8.443	3.149	5.298	11.596
7.735	8.333	8.632	4.519	4.113	13.151
5.328	7.250	8.211	3.285	4.926	11.496
7.657	8.833	9.421	3.720	5.701	13.141
6.434	8.2500	9.158	3.400	5.758	12.558
5.750	8.250	9.500	4.158	5.342	13.658
8.526	9.000	9.237	3.525	5.712	12.762
8.076	9.166	9.711	4.289	5.422	14.000
5.658	8.500	9.921	4.334	5.587	14.255
7.815	9.833	10.842	4.247	6.595	15.089
8.342	9.500	10.079	2.999	7.080	13.078
8.268	9.300	9.816	3.753	6.063	13.569
9.420	9.666	9.789	3.695	6.121	13.511
7.342	8.500	9.079	3.525	5.554	12.604
7.078	8.500	9.211	3.896	5.315	13.107
7.09	8.100	8.605	3.090	5.515	11.695

writing-pressure values. Table IX and Diagram E (Fig. 2) give the results of the third consecutive run for the same 38 *Ss*, the runs being one week apart, and Table X gives the corresponding writing-pressure measurements. Diagram F shows the reaction-times for the 20 *Ss* who were the most rapid in addition of the digits, and Diagram G shows the reaction-times for the 21 *Ss* who were the slowest in ability in the problems. In these latter two diagrams the reaction-times have been multiplied by a factor to reduce their curves approximately to the same level. Diagram H represents the author's conception of the effect of distraction.

Evidence of automatization of problem-solving does not seem to be present in a single sitting. There is no increase in the speed of working in the first 6 problems of the 'normal' period; and there is no appearance of a lowering of the level of the means in the entire run anywhere. But when we examine Tables III, IV and IX and Diagrams C, D, and E (Figs. 1, 2) we note that in the successive runs for the same *Ss* there is a distinct increase in the speed of working the problems, accompanied by a distinct decrease in the number of errors. It appears that the lapse of a week between sittings seems to bring out a tendency toward auto-

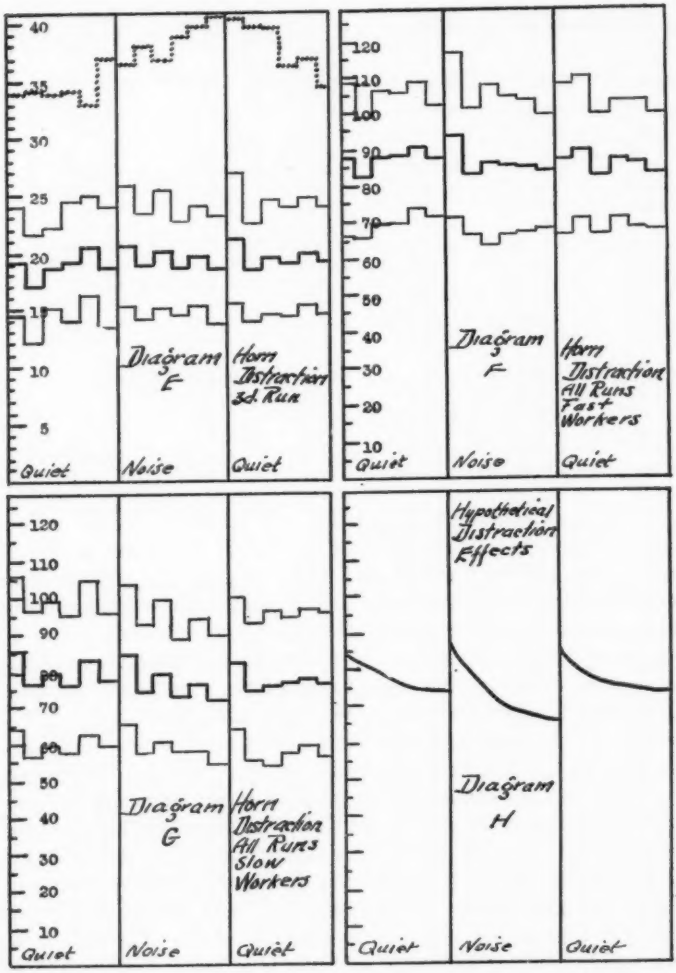
40
35
30
25
20
15
10
5
Quiet
Noise
Quiet
Diagram E
Horn Distraction 3d. Run
120
110
100
90
80
70
60
50
40
30
20
10
Quiet
Noise
Quiet
Diagram F
Horn Distraction All Runs Fast Workers
120
110
100
90
70
50
40
30
20
10
Quiet
Noise
Quiet
Diagram G
Horn Distraction All Runs Slow Workers
Quiet
Noise
Quiet
Hypothetical Distraction Effects
Diagram H

FIG. 2

matization not observable in the course of one sitting. The first problems of the second sitting were always done more rapidly than the last problems of the previous sitting. If automatization is present in the first sitting it is obscured by the presence of fatigue or other unknown factors. The third consecutive sitting shows an initial reaction-time still shorter than the last problems of the second sitting. The difference between the first and second, however, is greater than the difference between the second and third, evidence that automatization is a process of gradual approach to a limiting level.

The most conspicuous point in the examination of the graphs of the tabular evidence is the initial slowing in reaction-time at the beginning of the noise-period and the beginning of the following quiet-period. The mode is thrown toward the center of the frequency curve in all cases and evidence indicates that the effect of the noise is on all reactions, not on a select few. This means that *both the noise and the quiet have constituted distractions.* But the initial slowing is more pronounced at the beginning of the noise-period than at the beginning of the following quiet-period. In all probability this is explained by the difference in the attitudes of the *S*s at the two different times. The automobile horn has certain meanings that produce stronger emotions than are found at the beginning of the last quiet-period. Apparently it is not the absolute presence of the noise that constitutes a distraction, but the fact that we have a change in the pattern-stimulus which changes the integration determining the reaction.

Introspective evidence came in great abundance. "When the quiet came on I missed the noise and wished it had not stopped." "When the horn stopped I was astonished because I had forgotten it was going," came in substance from 5 *S*s, although the horn was so loud that no other sound could have been heard. Other *S*s said that they "missed the racket," that they were bothered by the "uproarious quiet," that they could "hear the silence" and knew they were not doing as well as they expected. Often *S*s would report that they wished the noise would stop and when it did stop they wished it had not. Occasionally an *S* reported having liked the noise because it produced an exciting effect. Most of the *S*s were certain that they worked more slowly during the noise-period, and were quite sure they worked somewhat faster when the noise stopped, a conclusion which was somewhat divergent from fact.

Viewed from a somewhat different angle, the above results place an entirely new light on our method of defining a distraction. It means that the effectiveness of one pattern to disrupt another

is not at all a matter of absolute intensity but rather the presence of a stimulus gradient in a given unit of time. Whether the gradient is negative or positive, that is, descending or ascending, the mental effects on integration are much the same.

A summary of these curves would show the following points.

(1) The mean gets lower with each lapse of a week, but does not drop so much between the second and third sittings as between the first and second.

(2) The mean deviation gets constantly smaller and smaller. This may be considered the result from the dropping out of uncontrolled distracting influences and attitudes.

(3) Skewness diminishes during the distraction, an indication that the distracting effect is spread over the short reactions.

(4) A pronounced retardation is found at the beginning of the quiet-period after the noise-period. Both noise and quiet are distractive.

(5) Distraction seems to have very little effect on errors. Probably the *Ss* paid more attention to accuracy than to time. There are always more errors in the first half of a period, however, than in the second half.

(6) The effect of the distraction is about equal for rapid workers and for slow workers in the first problem of the noise and quiet. But the rapid workers make a very prompt recovery from the effects of the distractions, while the slow workers require nearly the entire 6 problems before the recovery is complete. Apparently the slow workers are unable to build up automatized resistance against the distraction.

VII. Writing-Pressure and Breathing

Morgan seems to have believed that the strength of the motor processes was in some way an index to the intensity of the mental events as differentiated from speed. There is reason to believe, in our experiment, that muscular tonus, once built up at the beginning of a distraction, tends to stay. Had we continued our sittings each over as long a period as did Morgan, there is a probability that we should have secured the same results. The general contention of Morgan, however, that the increase of speed of his *Ss* during noise-intervals was accomplished through the expenditure of an unusual amount of motor energy does not seem to harmonize with our writing-pressure curve.

Noting the graphs in Diagrams C, D, and E, it is quite evident that there is almost no correlation at all between the mean of the writing-pressure curve (the dotted line) and the mean of reaction-times. These are based on only 38 runs, however, and the samples are not large enough to show results free from chance variability. But if we combine all the sittings of the entire experiment, as they are illustrated in Diagram B, we have the rather surprising illustration of the statistical fact that when we add together several non-correlated pairs of measurements, as in Tables III, IV, and IX, and the measurements of 3 *Ss* not included in these tables, but whose data also showed no correlation, we may get a very reliable correlation between writing-pressure and reaction-time in the larger table. Using the formula

$$r = \frac{\Sigma xy}{\sqrt{\Sigma x^2 \cdot \Sigma y^2}},$$

we find that the correlation between the values expressed in Tables II and VI is 0.55. The cropping out of this correlation from the addition of the three smaller tables together, and the evidence not tabulated, can be explained by the fact that with enough cases the negative and positive deviations from the mean tend to cancel each other and leave values which are the real and universal effects of the distraction.

Since we have here a positive correlation between writing-pressure and reaction-time, and since we are assuming that the presence of long reaction-time is an indication of lack of automatization and of the presence of attention, it would seem that in the end we must abandon any theory that continuous and rapid work under noise conditions will be accomplished at a sacrifice of muscular energy. In all probability the functioning of motor tonus in some way or other facilitates the building of new integrations, and when these integrations are well enough established to work without interference from other pattern-stimuli, the reenforcement of the proprioceptive excitations from motor tonus is no longer needed.

The evidences of articulation in the breathing curves for our experiment resemble the descriptions given by Morgan and in all probability were as nearly identical as possible. We do not believe, however, that it is possible to produce as exact a ratio between inspiration and expiration as he seemed to give. Due to the fact that there were inhibitions of breathing frequently occurring, it was very difficult to decide whether a given portion of the curve was to be regarded as an inspiration or an expiration.

VIII. Vaso-Motor Phenomenon

While the various theories for the relation of the vaso-motor phenomenon to the fluctuation of attention did not concern us here to any great degree, we had thought at the beginning of the experiment that we might investigate the possibility of a connection between the strength of the phenomenon and the degree of attention, if our facts gave us a warrant for using the phrase "degree of attention."

A review of the early theories of the relation of blood circulation to attention has been given in Pillsbury's[20] work on attention. The studies and theories show the usual appearance of early work on new and complex organic principles. There seems to be a general tendency to regard the Traube-Hering wave as a simple phenomenon rather than complex. This tendency to make mental and physical processes too simple has permeated the entire history of man's investigations in the realm of biology. If the changes in mental integration we choose to call attention have anything to do with the changes of the Traube-Hering wave, it is certain that their part may be obscured by other strong forces of non-mental origin.

The early theories that the peripheral constriction of the capillaries and arteries is accompanied by a cerebral passive dilatation must be held with considerable doubt in the face of the more recent evidence that there may be active constriction in the cerebrum. Huber[21] found evidences of efferent fibres terminating in the cerebral capillaries. Wiggers[22] showed the possibility of active constriction in the cerebrum. Shepard[23] found evidences of correlation between vaso-constriction of the cranium, in trephined cases, and vaso-constriction of the periphery. We do not wish to treat of this controversy in detail, but merely to indicate that at the present time any anaemia theory of the relation of blood circulation to attention is certain to find strong opposition in some scientific quarters. We may not assume here with any great degree of safety that the adjustment of blood circulation is the cause of attention.

But there seems to be a considerable amount of evidence that the objectively controlled changes in attention may bring about a change in blood circulation. Angell and Thompson[24] reported certain characteristic shapes of the vaso-motor curve with mental events. If attention were to be considered as a state, unvarying and constant, and if there were a constant correlation between the amount of vaso-constriction and the degree of attention, we should expect to find the degree of constriction tending to take on a certain level and keeping that

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[20] W. B. Pillsbury, *Attention*, 1908, 75-80.

[21] G. C. Huber, Observations on the innervation of the intracranial vessels, *J. Comp. Neur.*, 9, 1899, 1-25.

[22] Wiggers, Action of adrenalin on the cerebral vessels, *Amer. J. Physiol.*, 14, 1905, 452-465.

[23] J. F. Shepard, *Circulation and Sleep*, 1914.

[24] J. R. Angell and H. B. Thompson, A study of the relations between certain organic processes and consciousness, *Psychol. Rev.*, 6, 1899, 32-69.

level as long as the attention remained constant. But anyone who has ever worked with the vaso-motor phenomenon knows that it is comparatively easy to produce a prompt constriction (the total change occurring often in as short a duration as three seconds) by all sorts of stimuli, but that it is very difficult to secure an equally prompt vaso-dilatation. The process seems to work rapidly only one way; the dilatation is usually slow when it is of the *active* (nerve-controlled) type. Passive dilatation may be secured just as rapidly as active constriction. We may note from Angell and Thompson's work that the most characteristic process is a rapid initial constriction followed by a slow dilatation for any and all stimuli except possibly the mental arithmetic, from which they did not seem to secure curves with uncontroversial evidence. But if the term attention cannot refer to a constant condition, but always becomes complicated with gradual automatization, then it is very possible that the degree of peripheral vaso-constriction is partially correlated with the degree of attention, assuming that the greatest degree of attention is secured only at the beginning of a work-period.

The invention of the Lombard-Pillsbury plethysmograph gave the laboratory an instrument which is very delicate in recording the capillary vaso-motor changes uncomplicated with the changes of the larger arteries; although it is perfectly true that the instrument is very difficult for quantitative calibration. Our use of this instrument indicates that the same type of curves holds for initial attention as were secured by Angell and Thompson for the more violent affective and emotional stimuli. We are exceedingly skeptical concerning the possibility of any attention experiment whatever in which feeling and emotional elements are well controlled, and are inclined to believe that the conditions for both affection and attention are identical.

During the fall and winter of 1919 we had occasion to accumulate 411 records of the vaso-motor phenomenon taken with the task of giving the fluctuating responses from stimuli of ambiguous perspective and binocular rivalry. About a fifth of these records were not of much value because of the fact that some of the *S*s displayed a continuous tendency to a strong constriction due to cold hands.

There were 91 *S*s in all, 26 women and 65 men. An *S* was placed on one side of a partition, faced either with a card on which was drawn the ambiguous figure or with the stereoscope in which there was placed a card with a pair of oppositely colored squares which are ordinarily used to produce retinal rivalry. The Lombard-Pillsbury plethysmograph was fastened to the forefinger of *S*'s left hand. The right hand moved a lever laterally to and fro to indicate the changes in fluctuation, which were all indicated on the smoked-paper kymograph drum. *S* was given a normal run, then a period of from 1 to 2 min. of fluctuation responses, then a rest-period of about 2 min., then another run giving the fluctuation responses. At the beginning of the experiment *S* began with the word 'start' and stopped at the word 'rest.' Sometimes there was a

terminal rest-period and sometimes there was not. We did not tabulate the vaso-motor phenomenon in the initial 'normal' period, and so we have 427 'work' periods tabulated as against 339 'rest' periods.

The following points were observed and statements of probability devised for each. (1) A rapid initial vaso-constriction followed by a slow dilatation. This meant that the constriction in the first 10 sec. must be more pronounced than at any other portion of the period. (2) A rapid initial vaso-dilatation followed by a slow constriction, or maintaining its level. (3) A level curve in which initial changes were either absent or so weak as to be unobservable. (4) An irregular curve in which the variations were deep but so constant that initial effects could not be observed. (5) Permanent constriction due to cold hands. (6) Vaso-constriction initially set up and maintained at a constant level throughout the period.

The situations above enumerated were as follows:

WORK-PERIODS
(427 sittings)

(1) 51% (2) 0.23 of 1% (3) 21.8% (4) 7% (5) 19.97% (6) 0%

REST-PERIODS
(339 sittings)

(1) 50.1% (2) 0.29 of 1% (3) 21.3% (4) 11.5% (5) 16.81% (6) 0%

The interpretation of the above shows that there is but one set of figures which warrants a positive statement; the tendency with a homogeneous series of acts for initial vaso-constriction followed by gradual dilatation. The statement is true of both rest- and work-periods.

Vaso-motor records from the distraction experiment using the motor horn as a distractor were more productive of results because of the greater reliability of the plethysmograph used. In 18% of the distraction records there was a one-to-one correspondence between problems and vaso-constriction from start to finish. In 66% of the records there were fragmentary periods of one-to-one correspondence, varying from 4 to 10 successive correspondences. The remaining records seem to show no definite correlation between the vaso-motor fluctuation and the separate problems.

In 183 distraction records there were 161 noise-periods in which there was an initial vaso-constriction stronger than in any other portion of the record. There were 94 of these records showing initial constriction in the last quiet period stronger than in any other place in that period. The measurement of vaso-motor effects during the distraction experiments merely verified the shape of the curve already attained from other evidence—that any *change* of mental processes, whether we call them attention, emotion, feeling, or what not, produces an initial constriction followed by gradual dilatation. Certainly at the beginning of the noise-period emotions were present, but this made no difference in the relative shape of the vaso-motor curve.

Although it certainly seems true that there were so many complex factors lending their forces to change the shape of the vaso-motor curve that we must be very cautious in using the Traube-Hering wave as an index to anything, it does seem that such interrelations as we have found favor a curve gradient as a concept of the nature of attention.

IX. Summary

Efficiency of work performed evidently goes through a definite transition as time elapses with environmental stimuli constant. Since names should not commit one to the assumption of faculties or other mental entities, we may use some name for the nature of that mental coördination which exists at the beginning of a new and novel stimulus combination, and because this coördination seems to have certain signs which resemble the conditions described by past workers in the field of psychology as *attention* we feel justified in whatever violence we may perpetrate in using the same name for the beginning of a synthesis of reactions to a novel environmental pattern. But because the following reactions seem to display definite differences in their capacity to show efficiency in the face of foreign distracting stimuli, we wish to give another name to the behavior which follows, and again we may borrow from the field of psychology that term which suggests past evidence resembling as closely as possible the evidence in our own work. The term *habituation* has been used, but without a clear enough distinction from the word attention. The word automatization has been used in the field of abnormal psychology as indicative of behavior without consciousness or behavior without attention. Again we may not quibble concerning the justification of the use of the term *consciousness*. The behavior in the later parts of the distraction-period and the behavior of a person exhibiting automatisms shows a similarity of form and conditioning. Possibly we may say that it tends to become unconscious, and therefore unattended. From a purely behavioristic point of view we have the certain distinction that initial reactions are highly susceptible to interference from new and heretofore unexperienced stimuli. Conversely, later behavior of the same type is capable of a certain capacity to resist interference from sudden and novel stimuli. The transition from the one to the other may be called the psychodynamic gradient.

The evidence for such a concept of attention-automatization may be listed as follows.

(1) In every case the initial reaction is always longer than any other reaction of the period having constant environmental stimulus patterns. This is true of both quiet and noise periods and in excess of any training effect.

(2) There is a slight tendency for more errors to be committed in the first half of a given homogeneous period than in the second half; although there were not enough cases of error in our evidence to warrant problem-to-problem comparison.

(3) The writing-pressure and reaction-time showed a reliable positive correlation of 0.55, indicating that new integrations of behavior were accompanied by evidences of general motor tonus, and, as time passes, this tonus likewise exhibits signs of a gradient. Motor tonus becomes somewhat of an index to the degree of attention, *as we have defined attention.* At this point it is well to warn the reader that this statement of the fluctuation of motor tonus is not entirely in agreement with Morgan's statement in that he apparently holds that the tonus continues through the distraction-period and that the general average of high efficiency for that period is due, not to automatization, but to some intense mental process which he does not name.

(4) Vaso-motor concomitants likewise show initial effects which go through gradient changes as the same type of work continues under constant environmental stimuli.

(5) Fast workers, probably individuals who, either from natural aptitude or from past experience, very rapidly acquire automatization of behavior in this work, show almost an instantaneous recovery after the first problem under the noise. Slow workers acquire a facility only gradually, a fact which appears reasonable in view of the possibility that the slowness may be due to inadequate past experience or the fact that, in acts of skill, speed is the accompaniment of automatization. The recovery from the initial effect of the distraction did not fully occur until the last problem of the series. Shortness of reaction-time apparently becomes a measurement of automatization whether we are comparing different records of the same individuals or composites of records from different groups. The curve-gradient is the most conspicuous element in every picture of temporal progress.

A SIMPLE SERIES OF ABILITIES

By EDWARD S. ROBINSON, Yale University, and
FLORENCE RICHARDSON-ROBINSON

And it should always be the aim of the experimenter not to revel in statistical methods (when he does revel and not swear) but steadily to diminish by continual improvement of his experimental methods, the necessity for their use and the influence they have on his conclusions. Statistical methods are not only ancillary; they are to the experimenter, a warning of failure. ...The experiment (if it can be called an experiment at all) is often not only bad, but its badness seems to be accepted merely as a rather interesting fact, calling perhaps for the exercise of more elaborate statistical methods—a few partial coefficients of correlation—a few rather elaborate probable errors—and that is all.—G. U. YULE

SITUATION IN DIFFERENTIAL PSYCHOLOGY

Nothing could have been of greater service to the prestige of psychology in the work-a-day world than that medley of grammar school exercises and parlor puzzles, the test of general intelligence. Although psychologists themselves have no very clear understanding of why it does what it does, a test of this type will uncover in an hour approximately as accurate information about the aptitude for study of a college freshman as his secondary school teachers could secure from a year or more of casual observation. It will tell more about the capacity of a candidate for a clerkship than will most letters from former employers. Such accomplishments should not be taken lightly. They are real; they are useful.

But technological prosperity is only one of the ends of science. As long as such prosperity rests upon a basis of rule of thumb, the conscience of science must be uneasy. Differential psychology is bristling with practical devices, but one sometimes doubts whether these devices are based upon more than a rule of thumb. On one point there must be agreement: the general theory of differential psychology has followed fairly closely in the trail of what has proved useful. The most ambitious attempt to set this domain into conceptual order, that of Spearman, has kept well within the atmosphere of practical testing. Perhaps it is well that this is so. On the other hand, it seems quite possible that the cause of theory might be benefitted if the conventional mental tests and the practical categories of intellect, skill, and character were occasionally

* Accepted for publication April 7, 1928.

neglected in favor of better defined, though more austere, procedures and conceptions. Where the issues and instruments of practicality are dominant, the ideals of experimentation are likely to receive scant homage. As Yule reminds us, the introduction of partial correlation and other elaborate statistical machinery as substitutes for experimental control ought always to be tolerated with regret.[1]

Plan of a Simple Experiment

We shall present in this paper a study of a series of abilities which have a simplicity and a susceptibility to experimental control conspicuously and almost necessarily absent in applied psychology. We shall also offer, in the light of our results, a discussion of some of the larger problems of differential psychology.

When we come to describe our specific experimental conditions there may be doubt about the legitimacy of our conception of *ability*. There may be a question as to whether *ability* should refer to capacity or performance. Popular usage surely implies something of capacity. It is common practice in scientific work, however, to deal with abilities merely as with directly measurable performances. This will be our procedure in the present study.

We shall mean by *an ability* average performance in copying a standard line of given length under given experimental conditions. What justification have we for including within one ability a number of separate performances? What justification have we for breaking up *ability to draw lines* into *abilities to draw lines of various lengths?* Our answer is that we have no *a priori* law by means of which the single ability can be distinguished and delimited. If this is true, the essential obligation is simply that one be clear as to what, in a given investigation, is to be meant by *an ability*.

We measured the abilities of 81 *Ss* in copying as accurately as possible standard lines of 10 different lengths: 6, 9, 12, 15, 18, 21, 24, 27, 30, and 33 cm. Later references to 'ability 6,' 'ability 9,' 'ability 12' will be understood to identify the abilities in terms of the various standards.

Although we know much less than *all* about the 10 abilities in question, we do know *something* about them, and that something is, in itself, quantitative and unequivocal. Furthermore, we have

[1]G. U. Yule, Critical notice, *Brit. J. Psychol.*, 12, 1921, 106.

this quantitative and unequivocal knowledge prior to the calculation of any coefficients of correlation. It should, therefore, provide a basis for the interpretation of such coefficients when they are brought forward. The situation has striking similarity to those under which psychophysical limens have been investigated. In psychophysics there has been a constant recognition of the fact that *just noticeable differences* and the like can receive elucidation only to the degree to which there has occurred some prior and unambiguous quantification of a simpler aspect of the total situation. Indeed, our experiment is essentially within the domain of psychophysics, since the pre-psychological quantification is of a physical order. We believe, however, that chief importance lies in the more general fact that there is laid, as a basis for the understanding of such coefficients of correlation as may turn up, a clear-cut, pre-correlational description of the abilities with which the experiment is concerned.

The *S*s were all undergraduate women in the University of Chicago. They were tested individually by F. R.-R.

Each *S* drew 40 lines for each standard, that is to say, a total of 400 lines.

The standards were presented in irregular order. The 10 lengths were given 20 haphazard arrangements and these arrangements were set in serial order. This accounts for a total of 200 presentations. The second 200 presentations consisted in a repetition of the first. This procedure distributed the 10 lengths evenly over the total series and yet provided enough irregularity to eliminate the possibility of the *S*s anticipating a standard before its actual presentation.

The standard lines were drawn on a long strip of heavy manila paper which was mounted on double rollers in such a way that one line at a time could be made to appear at a slit in a cardboard screen. The paper was 45 cm. wide and the slit had a length of 50 cm. and a width of 4 cm. The screen was on a table and the exposure slit was at about eye-level for the seated *S*. Distance from eye to screen was 14 to 20 cm., according to the preference of the *S*. In practically all cases this distance was close to 19 cm.

The paper upon which the *S* drew was mounted on a roller under the table. From this roller it was drawn up and turned over the front edge of the table. When the *S* had drawn one of the lines she was able to push the paper strip forward in such a manner as to carry the drawing under the exposure screen and out of her sight. She was then ready for the next line which was turned into place at the exposure slit. The paper on which the drawings were made was 61 cm. in width.

The *S* was permitted to work at her chosen speed. The average time required to draw the 400 lines was about 45 min. with extremes at 30 and 60 min.

Instructions. The verbatim instructions were as follows:

"I want you to draw on this strip of paper lines of the same length as will be shown you on this screen. Pay attention only to the *length* of the line. There is no trick or illusion about it.

"Do not try to figure out to yourself the length of the line. It will only confuse you. The lines are not drawn in inches.

"Look at the line once; draw it in a single stroke, push the paper under the screen, and be ready for the next line.

"Do you understand it?"

Results

The data upon which the present paper is based were secured by measuring all of the 81x400 lines, each to the nearest millimeter. All of these measurements were made by one well-trained and exceedingly accurate assistant. We made no subtractions of produced lengths from standard lengths or vice versa. For each *S* we determined simply the average length of line drawn for each of the 10 standards. N for each *S*'s average for any given length was thus 40. As a basis for self, or zero-step, correlations we also averaged for each *S* the odd and even drawings within the 40.

Our raw results, consisting of 32,400 separate measurements, are not given in this paper. These data are on file in the Yale laboratory and will be made available to any investigator who wishes to utilize them.

In Table I are presented the complete averages for the 81 *S*s. These are the averages involving the 40 drawings for each length. The averages for odds and evens are not given here, but they also are on file.

The *abilities* with which this analysis will deal have an important peculiarity that should be noted. The term *ability* usually refers to a level of capacity or achievement of such a type that high values reflect worth or skill. A high value among our data represents merely the production of a line longer than that produced on the average for a given standard. Whether this greater length implies greater or less approximation to the appropriate standard is completely neglected so far as the present investigation is concerned.

Our first and major interest is in Table II, which contains the r's for all of the pairs of abilities involved in our experiment. The coefficients for each ability and itself are the result of correlating odd with even drawings. The self-correlations as given in the table have been corrected through the application of Spearman's well-known prophecy formula which in this case is, specifically,

$$r_x = \frac{2r}{1 + r}.$$

The main task is that of determining the influence of differences in length of standard upon the intercorrelations. Since each ability is correlated with 9 other abilities as well as with itself, it is possible to think of the abilities as having places in 10 distinct functions. On this basis the entire set of 10 abilities can have 10 different internal arrangements according to whether the point, which might appropriately be called the point of *correlational perspective*, is at one or at another of the abilities in question. In Figs. 1–10 we have plotted these 10 functions.

TABLE I
AVERAGE LENGTHS DRAWN BY EACH SUBJECT
n for each average = 40

	Length of Standard (mm.)									
S	60	90	120	150	180	210	240	270	300	330
1	69	109	146	180	211	250	281	317	342	366
2	75	108	133	163	189	225	257	289	314	345
3	70	94	131	161	187	230	268	308	327	359
4	69	98	133	159	184	216	248	280	307	325
5	103	140	154	178	199	237	259	290	309	332
6	85	116	160	179	196	247	287	307	337	362
7	73	111	142	177	204	228	256	281	301	331
8	75	110	142	173	205	243	276	307	331	359
9	65	93	121	157	186	219	252	283	315	347
10	87	123	156	185	211	234	259	287	309	337
11	80	109	141	160	187	233	260	292	310	341
12	76	109	138	173	195	229	257	287	318	348
13	71	94	119	141	161	194	211	237	278	305
14	78	106	135	158	183	211	239	273	302	330
15	81	121	150	179	199	239	260	288	317	332
16	68	101	136	174	203	241	277	307	333	367
17	79	106	135	162	193	225	270	301	346	372
18	72	107	135	167	196	230	262	303	327	358
19	64	103	133	161	183	207	242	270	303	333
20	71	101	136	165	185	225	262	296	330	360
21	97	136	160	188	204	254	278	317	340	365
22	65	93	126	151	190	230	263	303	332	348
23	75	105	136	168	194	236	270	304	331	363
24	54	76	106	130	147	170	185	197	223	235
25	77	108	146	179	214	248	278	308	334	360
26	54	88	128	154	191	215	240	266	299	323
27	78	119	148	179	203	244	280	306	337	364
28	77	108	143	170	193	227	256	280	299	317
29	71	114	140	169	201	228	254	285	322	355
30	67	94	126	154	182	219	248	275	300	330
31	76	105	134	159	182	215	237	264	289	318
32	67	105	138	172	203	233	259	291	318	336
33	64	94	121	144	159	200	227	266	297	332
34	88	115	148	184	209	250	285	309	337	364
35	71	105	141	169	193	228	252	283	319	350
36	67	100	131	163	185	218	248	286	314	346
37	71	114	143	176	200	225	257	286	311	338
38	72	103	130	162	182	225	253	287	306	338
39	67	97	128	158	182	217	252	281	317	345
40	70	95	121	153	177	216	250	283	295	332
41	69	100	128	161	185	212	236	268	304	338
42	87	118	155	178	198	246	266	288	321	351
43	64	93	132	161	188	238	272	319	357	400
44	74	106	139	170	204	237	271	303	330	361
45	76	106	134	166	194	224	255	293	326	345
46	78	103	138	171	194	234	268	298	324	354
47	77	107	137	161	185	240	272	310	330	364
48	74	102	125	154	179	207	235	267	292	321
49	83	116	150	184	207	243	274	304	331	356
50	83	114	145	181	213	251	273	311	331	361

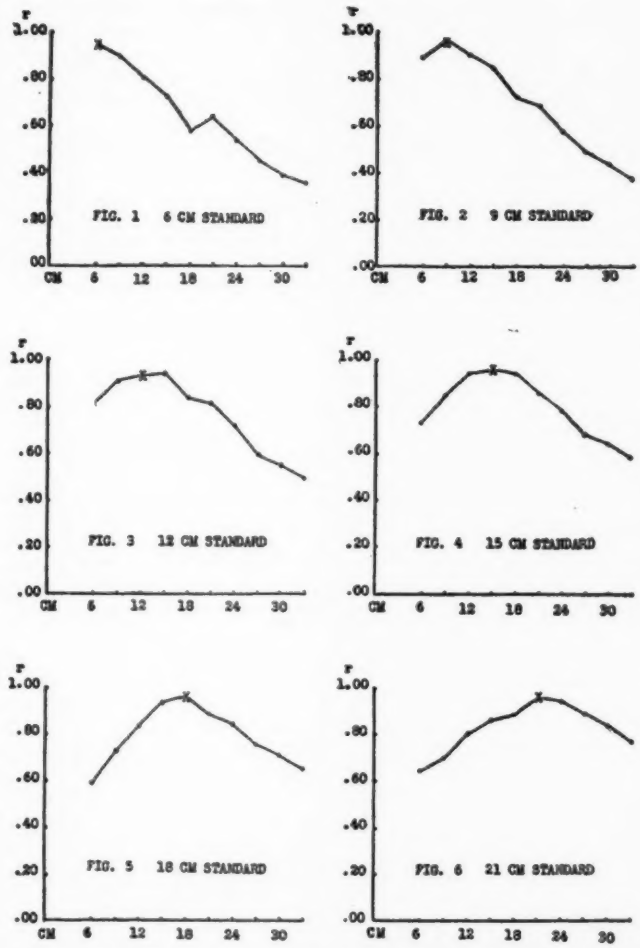
TABLE I (Continued)
AVERAGE LENGTHS DRAWN BY EACH SUBJECT
n for each average = 40

	Length of Standard (mm.)									
S	60	90	120	150	180	210	240	270	300	330
51	75	107	145	171	197	235	271	293	328	354
52	81	125	154	183	200	230	252	278	300	319
53	90	124	151	184	207	245	273	302	326	355
54	68	94	125	157	187	224	264	303	332	363
55	69	97	128	159	178	207	226	250	269	310
56	79	113	142	176	206	249	276	295	330	359
57	68	97	124	151	176	221	248	282	292	314
58	72	104	136	165	192	228	258	285	315	344
59	66	104	129	159	183	218	243	275	301	316
60	64	101	135	168	195	218	248	274	301	336
61	69	101	137	171	197	231	268	296	323	354
62	86	122	166	194	221	247	278	296	329	355
63	66	100	138	169	188	223	263	301	330	352
64	67	95	129	162	184	215	249	281	306	335
65	73	104	135	162	186	223	260	285	319	333
66	93	130	169	201	223	275	299	334	360	388
67	80	111	145	177	208	239	267	295	323	353
68	90	122	154	188	218	260	285	314	343	364
69	66	101	136	159	181	218	246	274	299	325
70	67	107	142	181	214	248	284	312	338	365
71	72	101	133	162	188	236	265	299	326	350
72	75	105	130	156	174	200	222	246	280	310
73	66	99	124	156	184	224	252	288	324	354
74	71	114	146	175	202	234	262	291	317	343
75	70	104	138	171	201	232	264	291	320	351
76	89	119	153	181	210	238	274	308	332	364
77	66	95	123	155	183	224	252	287	314	336
78	78	111	137	172	200	229	262	292	319	352
79	76	111	138	174	195	221	263	307	338	373
80	80	111	140	174	206	240	271	301	330	359
81	70	106	138	171	205	237	274	299	333	363

TABLE II
VALUES OF r AMONG THE VARIOUS ABILITIES

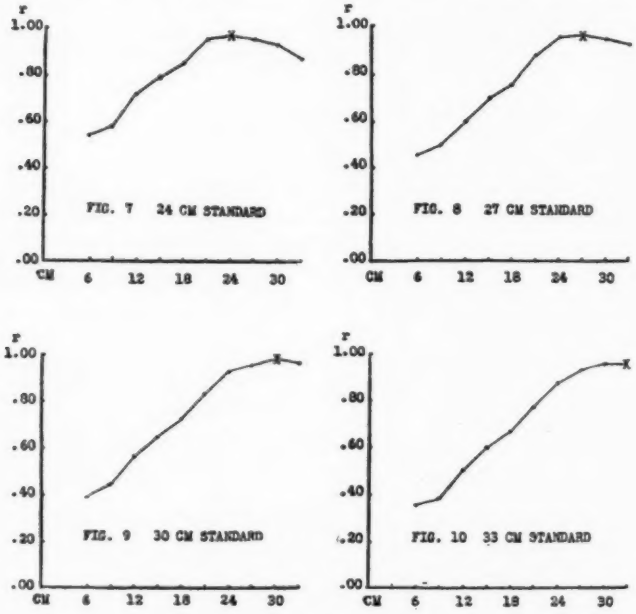
(These product-moment coefficients were calculated under the direction of Professor Clark Hull at the National Correlation Bureau, Madison, Wisconsin)

Ability	6	9	12	15	18	21	24	27	30	33
6	.9558	.8975	.8024	.7225	.5866	.6391	.5279	.4551	.3968	.3567
9	.8975	.9615	.9034	.8534	.7225	.6954	.5735	.4913	.4403	.3831
12	.8024	.9034	**.9322**	.9389	.8325	.8092	.7181	.5998	.5641	.4985
15	.7225	.8534	.9389	**.9521**	.9317	.8576	.7897	.6844	.6425	.5929
18	.5866	.7225	.8325	.9317	**.9622**	.8861	.8430	.7479	.7179	.6646
21	.6391	.6954	.8092	.8576	.8861	**.9677**	.9472	.8848	.8325	.7752
24	.5279	.5735	.7181	.7897	.8430	.9472	**.9691**	.9556	.9213	.8731
27	.4551	.4913	.5998	.6844	.7479	.8848	.9556	**.9746**	.9554	.9236
30	.3968	.4403	.5641	.6425	.7179	.8325	.9213	.9554	**.9803**	.9652
33	.3567	.3831	.4985	.5929	.6646	.7752	.8731	.9236	.9652	**.9647**



FIGS. 1–6. Each figure represents the varying values of r between a given ability and all of the other abilities. The cross marks the self-correlation and thus indicates the ability with which the others are correlated. The several abilities are represented by divisions of the base line and values of r by the divisions of the vertical axis.

As one would naturally expect, the self or zero-step r's, as corrected, are the high values. The one or two exceptions are unimportant. As the other standard lines differ more and more from that standard which, in a given case, is the point of correlational perspective, the r's drop off pronouncedly in magnitude. At the zero-step we have about as close relationship as is ever attained from separate psychological reactions. Coefficients obtained where there is a linear difference of 15 cm. or more are of about the magnitudes obtained among the variables investigated in practical studies of intellectual competence. If one must have a conclusion as to whether our functions are or are not steep, we may say that they are steeper than we had expected. Indeed, at the time the



FIGS. 7–10. See legend Figs. 1–6.

conditions of this study were laid down, but before any results were in, we described the situation to a psychologist who usually shows great skill in estimating relationships to be measured by means of the correlational method. His estimate was that all of our r's would be high, as r's go, and that there would be such small differences among them that we should not be able to distinguish separate functions for the 10 perspective points. After results like these are in they seem a bit obvious, perhaps, but we believe that a preliminary question-

nary would have collected from the most expert an astonishing number of bad guesses. All of this proves merely that there is a great deal yet to be learned about the organization of human abilities.

There is usually a strong interest in the business of predicting status in one variable from known status in another. It has been pointed out repeatedly by Kelley and by other authorities that r is by no means a ratio of knowledge to ignorance in such prediction. Such a ratio is much better expressed by k, the coefficient of alienation.[2] We have calculated the k's for our data. They range in fairly orderly fashion from about 0.27 at the zero-step difference to about 0.93 at the extreme difference. This would mean that our prediction of a set of performances with one standard from knowledge of another set of performances with the same standard would be about 7/10—(1.00 − 0.23 = 0.67)—as accurate as would prediction on the basis of perfect knowledge. But with a difference of 27 cm. between the lines in question prediction from one to the other would be only about 7/100 as good as if based upon perfect knowledge.

These questions of absolute degrees of relatedness among the variables of our experiment are not of great theoretical importance. However, since we know so little about the degree to which actual abilities are related, these absolute values, secured as they have been from data of unusual statistical certainty, do add their mite to our knowledge. It would, of course, be interesting and important if the rate of fall of our r's could be compared with analogous rates in the case of coefficients existing for groups of intelligence tests.[3] The possibility of strict comparisons is eliminated by the fact that there is no way to plot r's from intelligence tests against cm. It is usually difficult to find *any* unequivocal units against which to plot changes in the values of r among the so-called intellectual performances. A difficulty in the way of even the roughest qualitative comparison hinges upon the difference in the levels of superficial resemblance among customary groups of intellectual abilities, on the one hand, and among our group of line-drawing abilities, on the other. It is quite possible that a continuation of our obtained functions much beyond the extent of those representing the greatest linear distances would reveal a deceleration so marked as practically to eliminate the influence of length-difference upon r. If this took place before r had fallen below the magnitudes typical of it when based upon the usual intellectual tasks, we should have to conclude that the specificity of these motor abilities is not unusual. But, since such continuation is lacking, it is impossible to make any very general statement about the relative rate of decrease of r as demonstrated by the present experiment.

The r's of Table II and the functions (Figs. 1–10) into which they can be arranged constitute, on first thought, a very complicated grouping of the 10 relatively simple abilities with which we are concerned. If the interrelationship between any ability and any other can differ according to what other ability is

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[2] $k = \sqrt{1 - r^2}$. H. E. Garrett (*Statistics in Psychology and Education*, 1926, 288 ff.) has a clear discussion of k, its meaning and its use. He also has references to the more authoritative discussion by T. L. Kelley.

[3] We cite intelligence tests only because their frequent use in correlational studies renders them familiar to all students of differential psychology. The same is not so true of motor tests, character estimates, and other devices of measurement.

used in evaluating or identifying the abilities in question, then the general form into which all of our facts can be brought will be a graphic figure of three dimensions. Height of the figure at any point will represent the magnitude of r and the other two axes will be cm. scales. One of the cm. axes will represent what we have called the points of correlational perspective, that is the linear magnitudes identifiable with the abilities with which the other abilities have been correlated. The other cm. axis will represent the linear magnitudes identifiable with the 10 individual abilities. These two axes happen, of course, to be alike in extent and in units. But the fact that the functions of Figs. 1–10 are not coincident shows the need for using these two identical scales in disparate dimensions. The reader can get a clear notion of the type of three-dimensional figure that will result if Figs. 1 and 10 are brought together. Proceeding into the third dimension, which represents the correlational perspectives taken from abilities involving greater and greater linear magnitudes, one would find the successive transverse sections of the solid figure to be planes bounded by the successive functions from 1 to 10. Or, to put the matter somewhat differently, the solid in question could be secured if the curves of Figs. 1–10 were cut out and placed one back of the other in the above order and the intervals then filled in.

Now such a tri-dimensional figure gives a very complete account of the inter-correlations among our 10 abilities. Tri-dimensional relationships are so difficult to keep in mind, however, that it would seem desirable to attempt some kind of a summary of the facts involved.

As one examines the curves of Figs. 1–10 there are seen to be certain similarities running through them. In the first place, the general slope is not greatly different for the different curves. In the second place there is evidence of a considerable stretch of what is practical linearity among those curves which take in the greater distances from the point of maximum correlation. In the third place, there seems to be a slight acceleration of fall a short distance beyond the point of maximum correlation and a slight deceleration at the other end of the curves. The situation apparent upon inspection can better be summed up, perhaps, by the statement that the curves falling in one or the other direction from the maximum would, if sufficiently extended, tend to take the form of the curve of Fig. 1 (without its apparently accidental central inversion) and that of Fig. 10. Our general estimate can be expressed in the negative proposition that there appear little grounds for dividing these curves into discrete types.

If the functions with which we are here concerned had been distinguishable into discrete types, we should have had to remain content with the tri-dimensional organization of them which has already been suggested. Under the existing circumstances generalization through averaging seemed justified. Accordingly, we calculated the average value of all the self-correlations. This average we call the zero-step average. We then obtained the average of all those r's resulting from cases where the linear differences between the members of the pairs of abilities were 3 cm., plus or minus. This we call the one-step average. After calculation of the averages up to that at the maximum linear distance, we had a series of 10 average r's that could be plotted against linear differences represented by steps from zero to plus or minus ten. This function is displayed graphically in Fig. 11.

The curve of Fig. 11 has the characteristics that seemed, upon inspection, to run through the 10 less generalized functions. The fact that it is so smooth is, in our minds, the best evidence that it is a fair summary of the curves derived from special points of correlational perspective. If the reader has considered exactly what was done in arriving at this curve of averages he realizes that the individual curves extending only 4 or 5 steps in either direction are, therefore, unrepresented at the 6th, 7th, 8th, and 9th steps. The ten-step average was, of course, derived from a single value, occurring in only 2 of the special curves, that is, the r for abilities 6 and 33. The fact that, in spite of all this, the average function is so regular would constitute a fairly adequate defense for that average as a summary of the special functions. The unequal representation of the special functions would have been likely to distort regularity if a tendency toward a common form had not permeated the individual curves.

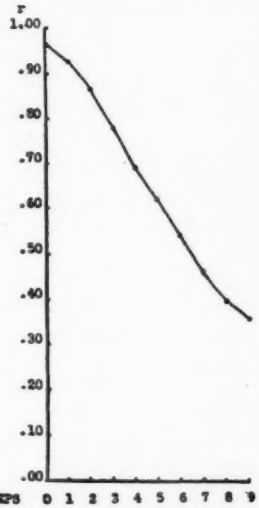

FIG. 11. Varying values of r for varying amounts of length-difference between correlated abilities. Base line represents amounts of length-difference in steps of plus or minus 3 cm. Vertical axis represents values of r.

We may now return to our primary question: What is the influence of the magnitude of the difference between any two standard lines upon the magnitude of the intercorrelation between the abilities identifiable with those standards? The answer is that r varies from about 0.35 to about 0.96 with decreases in the length-difference within the limits studied. There is no evidence, however, that the limits of our study have taken in anything like the total range of effectiveness of this variable length-difference. As a factor determining correlation its influence is, therefore, surely and considerably greater than is represented by the variation of r within the range from 0.35 to 0.96.

Furthermore, from the fact that the curve of average values (Fig. 11) has astonishing regularity, it seems justifiable to abstract the relationship between r and length-differences from the particular differences investigated.

There is little evidence that moderate extension of the lengths used would have greatly altered the influence of length-differences upon r. Naturally we should expect something interesting to happen if, on the one hand, the lengths used included very short standards, say as short as 5 mm., or very long standards which could not be drawn with a single sweep of the arm. We purposely selected lengths the drawing of which seemed to include movements similar in *type* though differing in magnitude. We are sorry now that we did not extend our range outside of the apparently homogeneous one actually investigated.

It would have been interesting to have seen whether apparent changes in type of movement would have been reflected in an alteration in the general function of Fig. 11.

As our preliminary remarks implied, the present study was undertaken not out of any great interest in the particular abilities investigated, but rather in the hope of demonstrating the possibility of a more definite type of analysis than is customarily employed in differential psychology. We shall now proceed to discuss certain general problems of individual differences from the standpoint exemplified by our experiment.

Conception of Factors in Correlation

The concept of *factors* appears in the consideration of one of the basic problems of correlational analysis, the problem of accounting for the degree of interrelation between two abilities. What we actually have given is an ability, A, and an ability, B, and the knowledge that $r_{AB} = X$. We are called upon to determine the facts or factors that cause this r to be as large as it is and no larger.

The most obvious move toward the solution of this problem is a scrutiny of the tasks defining the abilities. An enumeration may be made of features apparent in both A and B and of features apparent in one or the other, but not in both. In order that this typical procedure be given very specific illustration, let us suppose that A is the ability to draw from a standard of 6 cm., and B the ability to draw from a standard of 9 cm. X, the value of r, here becomes 0.898 (see Table II). But why is it so high? If we knew only A and B and the correlation between them, we should probably begin an enumeration of the kind mentioned. Let us see where it would lead us. The features common to A and B are, among countless others, visual acuity, use of fingers, use of forearm, capacity to resist distraction, desire to do one's best, comprehension of instructions, etc., etc. These are some of the most obvious of the common features and it would seem on first glance as though such obvious similarities must have a great deal to do with the fact that r is in this case so high. Now, if it were not for the fact that we are put on our guard by our knowledge of other correlations into which these abilities enter, we should be inclined to accept these enumerated common features as important *factors* in the correlation between A and B. But having the knowledge we do have, we are forced to the almost opposite conclusion that, so far as the factors enumerated are concerned, r might as well have been 0.357 as 0.898. The features pointed out as common to A

and B are just as truly common to A and to an ability having a correlation with A of 0.357, that is, common to drawing to a 6 cm. standard and to a 33 cm. standard.

It has frequently been noticed that because two abilities can be subsumed under *memory* or *language* or some other category of that order is no reason why the r between them need be high. But we believe it less often noticed that the superficial similarities of highly correlated abilities may have almost nothing to do with the magnitude of the r whose explanation is being sought.

An advance over the explanation from inspection comes with the introduction of other abilities besides the two whose intercorrelation sets the problem. Let us suppose again that A and B are two of the abilities represented in our actual experiment, the drawing to a standard of 6 cm. and the drawing to a standard of 9 cm. Let us suppose also that we have 20 other abilities such as tennis playing, high jumping, sentence completion, rhyming, solving arithmetical problems, etc., and that these additional abilities are correlated in all possible ways with each other and with A and B. All of the abilities before us can be classified roughly into those involving mostly muscular skill and into those involving what psychologists call intellect. Having divided our abilities in this fashion we discover that the r's average higher among the abilities within one of these groups than they do when we include the r's obtained among abilities lying in separate groups. A and B obviously belong to the *muscular skill* group. Therefore we should expect a higher value of r than would occur if one of these abilities had belonged to the intellectual class. This explanation would be important if the r_{AB} were of about the same magnitude as one finds wherever the correlated abilities are muscular abilities. But the value of that r in this particular case happens to be in the neighborhood of 0.9 and the average r among 'random' samplings of muscular abilities, or among 'random' samplings of intellectual abilities, is probably never in that vicinity.[4] To explain an r of 0.9 by saying that it represents the correlation between two variables both belonging to a large class within which the r's seldom fall below 0.1 or 0.2 is to make slight progress toward any explanation at all. That A and B are both

[4]E. Garfiel obtained intercorrelations among a large variety of "motor" tests. The central tendency of her r's was from +0.15 to +0.25. Cf. *Arch. Psychol.*, 9, 1923, no. 62, 1–46.

muscular activities provides a *factor* in common, but a factor which almost surely has very little to do with the magnitude of r.

A somewhat different approach to the investigation of the magnitude of a given r makes use of the technique of partial correlation. Again the explanation of the correlation between two abilities is based upon knowledge of other variables and their correlations among themselves and with the abilities in question. The simplest use of the general method requires three variables. Having them, we may apply the widely used formula:

$$r_{12.3} = \frac{r_{12} - r_{13} \cdot r_{23}}{\sqrt{1 - r_{13}^2}\ \sqrt{1 - r_{23}^2}}$$

If the subscripts 1, 2, and 3 stand for three mental tests, Opposites, Analogies, and Synonyms, we are able to determine what the correlation between Opposites and Analogies would be if the ability to give Synonyms were kept constant.

Knowledge of the degree to which a given r would be reduced by keeping constant some factor common to the two correlated variables is unquestionably a forward step; but it is only a step. It is a long, long way from a description of those *gradual* variations in one ability which will increase its correlation with the other to 1.00 or decrease it to 0 or even to −1.00. We believe that the achievement of such a description implies the attainment of a degree of experimental control which would render partial correlation quite unnecessary.

The ideal *factor* of correlation is a variable condition demonstrably associated with various degrees of correlation. Length-difference in our experiment is such a factor. It can be defined in unequivocal fashion and it can be produced in perfectly definite amounts. This factor was made to produce an r of from 0.35 to 0.96 depending upon its own magnitude. The likely criticisms of length-differences as a factor of correlation would be in terms of its triviality and in terms of its lack of singularity. By triviality we refer to the fact that this variable has no immediately practical bearing upon life at large. By lack of singularity we refer to the fact that there may be other variables besides length-difference which have an influence in determining the value of any of the r's to which length-difference may be importantly related. This we should readily admit. Although one may have shown experimentally that a given r can be reduced from unity to zero through the

variation of some related factor, that does not mean that there are not other variables quite as effective. *Every definite variable whose relationship to a given r can be demonstrated is a factor of that r; and the only limit to the importance of any such factor is the range of values of r with which its own several values are associated.*

We are sure that this interpretation of the meaning of *factors* is at least partially applicable beyond the range where the psychophysical nature of the abilities makes easy the definition and control of basic variables. Suppose that our concern were with the r between Analogies and Opposites in the case of a group of 9-year-old boys. A probable factor in this particular r is age, and age could be controlled. So too could practice, which has often been shown to be of importance in determining the magnitude of r.[5]

Although a factor of correlation must be a variable, it may not be a continuous one. It may represent merely division into two or more discrete classes. For example, r for any two abilities might vary with the sex of the population manifesting those abilities, and in that case *sex* would be a factor of correlation. Of course, the continuous variable is the ideal one. But a qualitative distinction can, theoretically, be quite as potent a factor.

Grouping of Abilities

The drive to classify psychological abilities has been very great. The number of distinct abilities subject to definite description and experimental control is enormous. But with few exceptions, as in the studies of musical talent, the more describable abilities have received little attention by students of individual differences. Where differential psychology has kept relatively close to experiment little use has been made of that fundamental concept of differential psychology—correlation. The 'simple process' soon became of minor interest to early students of individual differences because their aims were fundamentally practical aims and they soon discovered that the simpler and more definable abilities gave poor predictions of practical performances. Thus we find much concern today with *Intellect*, *Muscular Skill*, and *Character*, regarding the definitions of which there is little agreement, and almost no concern with abilities that are definable though lacking, perhaps, the humanistic appeal.

[5]See especially G. S. Gates, Individual differences as affected by practice, *Arch. Psychol.*, 8, 1922, no. 58.

The groups of abilities with which differential psychology generally deals are fundamental to that psychology. The groups—particularly *Intellect*—are considerably more important than the observable abilities that they include. In the experiment reported in this paper, we studied 10 distinguishable abilities and we approached them with little or no notion of what grouping or groupings might develop upon the application of the correlational technique. Now, even though we attach no philosophical superiority to deduction upon the one hand or to induction upon the other, it may be worth while to consider the fundamental problem of grouping of abilities from the somewhat distinct point of view represented by our results. We feel sure that such consideration will bring old conceptions into new light, if it does not bring out some new possibilities in regard to the general problem.

The problem of grouping might be phrased somewhat as follows: From the standpoint of intercorrelation what grounds are there for bringing together several otherwise distinguishable abilities and for marking them off as a class from other abilities and groups of abilities?

If, in the case of our data, variations in length-difference were, over considerable ranges, unrelated to the magnitude of r, we should have a definite basis for gathering a number of abilities into one or a few groups. The abilities within one of these groups would manifest equal intercorrelations, or at least their intercorrelations would be independent of length-differences. Grouping by neglecting length of standard would be only one conceivable type of grouping, but, if based upon such statistical considerations as we have mentioned, it would certainly be a legitimate one.

Because of its manifest failure to allow for any unifying procedure in respect to these 10 determined abilities this citerion of equal intercorrelation may seem a foolish one. By a quite plausible, though hypothetical, extension of our empirical data, however, we believe it possible to describe circumstances under which unity based upon equal intercorrelation might well be possible.

Our best idea of the probable consequences of enlarging the range of linear magnitudes involved in our actual experiment are to be had from examination of the curve of Fig. 1. (The curve of Fig. 10 cannot be so well extended in the opposite direction because of the fact that it would soon run into a zero length.) If the linear magnitudes of our standards had been of much wider range,

the continuation of Curve 1 (Fig. 1) would have had either to decrease its rate of approaching an r value of 0 or else cross the zero-line. The extreme rarity of consistently negative values among r's representing even extremely divergent types of abilities would make it seem very likely that Curve 1 would ultimately show a much more marked deceleration of fall than the one actually obtained and that there would finally appear an almost horizontal section of the curve. This would mean that there were several abilities which correlated with our ability 6 to approximately the same degree. From the correlational perspective of ability 6, these other abilities would 'look alike' and might with a certain justification be classed together. Now this is, of course, only a partial fulfillment of the condition of equal intercorrelation that we originally had in mind, though we should say here that it is a perfectly defensible basis for classification. The abilities that would 'look alike' so far as their intercorrelation with ability 6 is concerned would presumably show marked differences in intercorrelation among themselves, just as do the abilities closer to 6. But, if all of the abilities studied were far enough apart along our base line, is it not conceivable that all of the possible intercorrelations would be approximately equal? In other words, if we considered only such abilities as would be represented upon any of the curves (Figs. 1–10) only after those curves became relatively horizontal, then we might have a close approach to the case of equal intercorrelation of which we have been speaking.

Fig. 12 represents the hypothetical situation. Three relationships between the magnitude of r and linear distance between the correlated abilities are given by curves a, b, and c. The points denoting self-correlation are marked by crosses. Thus, curve a shows all the r's for ability 5 and all other abilities, curve b all the r's for ability 10 and all other abilities, curve c all the r's for ability 15 and all other abilities. The lower limits of all three curves are about 0.20 and after reaching this vicinity all become practically (though not theoretically) indistinguishable. Under these circumstances abilities 5, 10, and 15 among themselves show approximately the same value of r and for that reason they can be grouped into a single class.

It is an amusing outcome of this particular type of unification that abilities, which, from the point of view of linear magnitudes, are most alike, resist inclusion in the same class; whereas abilities

which, from that standpoint, are very different will go into a single class. Thus, 5 will go into a group with 10, and 15, but 6 can not be put into that class because of its high correlation with 5. This by no means excludes 6 from entering into a general group. Presumably 6, 11, and 16 would show equal intercorrelations. This suggests, of course, that the criterion of unity here discussed would allow the formation of a large number of overlapping groups. If the starting point were 5, one set of abilities would be included; if it were 6 another group would result; and these two groups would show a large overlapping.

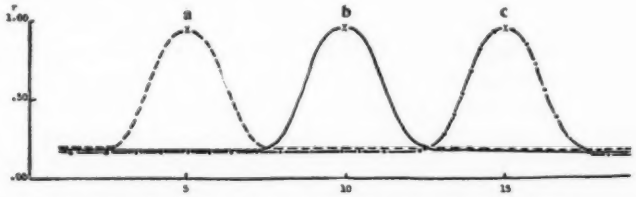

FIG. 12. An hypothetical case of relationships between linear differences and r where the range is very much greater than that in our actual experiment.

There is often an interest in determining, for a given set of r's, to what degree they are all brought about by some general common factor. Perhaps the practical coincidence of our various hypothetical curves at 0.20 represents from a new viewpoint just such a determination.

Another procedure for grouping together a number of abilities would be that of putting into the same class all of those abilities having more than some stated minimum correlation with some given ability, such as 5 in our Fig. 12. The curve, a, as it leaves the horizontal would mark one limit to the class and at its return it would mark the other limit. Or we might group around 5 only those other abilities reached before the positive accelerations in the falling curve.

As in the case of grouping on the basis of equal intercorrelation, grouping on the basis of more than minimum intercorrelation would make possible a large number of overlapping groups. The number of groups and their precise make-up would depend upon the particular abilities around which the other abilities were grouped.

The two types of grouping thus far discussed seem to us to correspond fairly closely with what is usually in mind when reference is made to *general abilities* and *special abilities*. Of course, the fact that our entire analysis rests upon the basis of a precorrelational quantification of the individual abilities makes rather hazardous any direct comparison between our analysis and those lacking in such a basis. We are inclined to believe that the usual notions of *general ability* do not include the idea of the overlapping of an indefinite number of such abilities nor the corollary that the specific constituents of one of these very general groups depends upon which one of many possible starting points (in the shape of some highly specific ability) is taken. We are also inclined to believe that the usual notions of *special ability* do not include the overlapping of such abilities which our analysis shows to be present.

The last type of grouping that we shall consider depends upon a somewhat more abstract conception than either of the first two. Inspection of the curves of Figs. 1–10 and the bringing together of those curves by averaging inclined us to accept the average curve (Fig. 11) as well representative of the relationship between r and length-difference throughout the range covered by our experiment. This would suggest, then, the possibility of throwing into one class all of those abilities to which a single law, such as that represented by Fig. 11, is applicable. If, within the limits of some single linear arrangement of abilities, orderly alterations were found in the average relationship between r and linear differences, it seems probable that those alterations would be so gradual as to make impossible any precise determination of the end of the applicability of one law and the beginning of the next. If that were true, the extent of any single class would depend upon arbitrary decision. Continuity would be outstanding; distinction would be largely a matter of convenience.

As we have gone over these various types of classification that are conceivably applicable to a series of human abilities, the persistent fact has been the arbitrariness of all of the suggested groupings. In our actual results and in our hypothetical extension of them 'natural' classes have been notably absent and continuous variation conspicuously present. Even under such circumstances there might be practical reasons why arbitrary groupings should be made. That is why we have discussed possible bases for such

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procedure. But when the practical need for grouping leads, as we believe it often has led, to the assumption that the needed groups must have some outstanding natural definition, there is a distortion of the probable facts.

Other Modes of Relationship Among Human Abilities

Our entire study has dealt with the interrelationships among actual and hypothetical abilities only in so far as these relations appear in values of the coefficient of correlation. Now, although the technique of statistical correlation is as important as any within the psychologist's equipment, yet it by no means exhausts the functional relationships among abilities. Transfer of learning and transfer of work decrement are two outstanding types of relationship which are not measured by the calculation of r. Such relationships may have a bearing upon r, but our understanding of the situation falls far short of permitting the prediction of one in terms of the other.[6]

A further essential characterization of our analysis is in terms of the fact that it has been, throughout, *social* in implication. We have said nothing about the relations among abilities 6, 9, and the others as they may occur in an isolated individual. It is a frequent practice in correlational studies which are really dealing with variations within some social group to couch interpretive conjectures largely in terms of the anatomical-physiological individual. We do not wish to make the negative claim that there is and can be no bridge from social correlation and differentiation to the largely non-social conceptions of anatomy and physiology (and also of general psychology), but we would insist that the bridge is still unconstructed and that the leap is full of peril.

Summary

Because students of differential psychology have had a great desire to render practical service to life at large, the investigation

[6]Some eighteen years ago Winch published a paper whose importance seems to us to have been too little recognized. In his investigation of transfer from one type of performance to another, Winch supplemented his data on transfer by obtaining intercorrelations between the performances in question, The lack of correspondence between these two equally legitimate measurements of psychological relatedness is striking and consistent. See W. H. Winch, Accuracy in school children: does improvement in numerical accuracy "transfer," *J. Educ. Psychol.*, 1, 1910, 557–589.

of logically and experimentally simple problems has been almost entirely neglected. The present paper has described an attempt to utilize the major method of differential psychology—the technique of statistical correlation—in a relatively simple and definable setting. The abilities chosen for study were such as to make reliable measurements possible. They were also open to unequivocal, quantitative description prior to the investigation of their intercorrelations, thus affording a quantitative basis for interpreting those correlations.

In light of our results we have discussed two fundamental issues of differential psychology: (1) the meaning of factors, or causes, of correlation; and (2) the nature of the grouping of simple abilities into larger classes.

We have stressed the claim that a factor of correlation is most ideally a mode of continuous variation which can be shown experimentally to be associated with rising or falling values of r. In regard to the grouping of abilities we have shown that a number of arbitrary methods are conceivable and possibly practical, but it is an outstanding feature of our own results that any sharp grouping is arbitrary rather than 'natural.' Continuity of variation from ability to ability is the rule.

THE LAW OF AFFECTIVE EQUILIBRIUM

By J. G. Beebe-Center, Harvard University

Recent experimental work has led to the formulation of many important *Gestaltgesetzen*—laws descriptive of the interdependence of parts within wholes. All of these laws, however, have dealt solely with cognitive or conative components of experience. The present paper seeks to extend the field of *Gestaltgesetzen* to include affective components of experience.[1] It describes two experiments the combined results of which show that the affective value of any one member of a sequence of experiences constituting a unitary temporal group is dependent in a definite way upon the affective values of all preceding members of the group.

I. Changes in the Affective Value of a Set of Stimuli as a Function of Repeated Presentation of a Part of This Set

Purpose. The purpose of the experiment described in the present section was to determine the difference between the affective values of a set of 21 olfactory stimuli as determined after repeated presentation of the 10 least pleasant members of that set, and as determined after repeated presentation of the 10 most pleasant members of that set.

Technique. The stimuli were 5 cc. of each of the olfactory substances listed below. These stimuli will hereafter be symbolized by the letters affixed to their names in the list.[2]

*Accepted for publication May 17, 1928.

[1]From the Harvard Psychological Laboratory. The writer wishes to acknowledge his indebtedness to Professor Edwin G. Boring for much guidance and assistance.

[2]These olfactory substances were procured from J. Méro & Boyveau, 25 Rue des Archives, Paris. Their French designations are given below to insure complete identification.

A = Essence d'Anis de France rectifiée
B = " de Portugal zeste extra
C = " " Girofle Amboine
D = Extraflor Cassie No. 30
E = Essence de Bergamote zeste extra
F = " " Cannelle Ceylan Importée
G = " " Jasmin synthet. s/fleurs
H = " " d'Ylang-Ylang Manille
I = " " de Cananga
J = Extraflor Oeillet Mignardise No. 30
K = Essence de Rose Synthet. s/fleurs
L = Essence de Genièvre baies
M = " de Petit Grain surfine
N = " " Geranium de Grasse
O = " " Fenouil doux
P = " " Neroli bigarade
Q = " " Muscade noix Algérie
R = " " Thym blanche de France
S = " " Patchouly mondée
T = " " Romarin éperlé
U = " " Cumin

A = Oil of Anis
B = " " Sweet Orange
C = " " Cloves
D = Extract of Cassie
E = Oil of Bergamot
F = " " Ceylon Cinnamon
G = " " Jasmin (synthetic)
H = " " Ylang
I = " " Cananga
J = Extract of Carnation
K = Oil of White Rose (synthetic)
L = Oil of Juniper
M = " " Petit Grain
N = " " Geranium
O = " " Fennel
P = " " Neroli Bitter
Q = " " Nutmeg
R = " " Thyme
S = " " Patchouly
T = " " Rosemary
U = " " Cumin

The *Os*, three in number, were Messrs. J. R. Butler, H. R. De Silva, and T. Karwoski. All the *Os* were graduate students of psychology. They will be symbolized hereafter by the first two letters of their surnames.

The approximate rank order of the 21 stimuli had been determined for every *O* in the course of previous experiments involving these stimuli. On the basis of this information there were constituted, in the case of each *O*, three sets of stimuli the characteristics of which were as follows:

(*a*) *Complete Set:* this set consisted of all the 21 stimuli (this set was the same for all *Os*).

(*b*) *Partial Set U:* this set consisted of the 10 stimuli of the Complete Set which were most unpleasant to the individual *Os*.

(*c*) *Partial Set P:* this set consisted of the 10 stimuli of the Complete Set which were most pleasant to the *Os*.

Fig. 1 illustrates this procedure in the case of *Ka*.

FIG. 1. *O* = *Ka*

The experiment consisted of four parts. Parts 1 and 3 were 'Determining Series,' *i.e.* experimental series the sole purpose of which was to determine changes in the subsequent affective judgments of the *Os*. Parts 2 and 4 were 'Test Series,' *i.e.* experimental series carried out to test any change in the affective judgments of the *Os* determined by Parts 1 and 3.

Part 1 consisted of 5 trials, each trial lasting one hour. During each trial the 10 stimuli which belonged for an *O* in Partial Set U (*i.e.* were the most unpleasant) were presented to that *O* in pairs, as in the method of paired comparisons, each stimulus being paired with every other stimulus in the set. The total number of presentations of each stimulus, during the 5 trials, was $5 \times 9 = 45$.

Instructions to the *Os* were as follows:

"There will be presented to you a series of successive pairs of smell stimuli. In the case of each pair, each stimulus will be presented for 5 sec. Sniff twice of each stimulus and judge which of the two experiences is the more pleasant. State your judgment in the terms: 'First more pleasant,' 'Second more pleasant,' or 'Equally pleasant.'

"In addition, please judge whether each of the experiences is absolutely pleasant, unpleasant, or indifferent. State your judgment in terms such as 'First pleasant, second unpleasant,' 'First indifferent, second pleasant,' 'Both pleasant,' etc."

As the sole purpose of Part 1 was to determine changes in the subsequent affective judgments of the *Os*, the data secured in Part 1 do not appear in the present discussion, although the judgments of the *Os* were recorded exactly as though Part 1 had been a 'Test Series.'

Part 2 consisted of 5 trials, each of one hour's duration. In the course of the 5 trials the 21 stimuli in the Complete Set were presented to the observers in pairs, each stimulus of the set being paired with every other stimulus of the set. Each stimulus was presented 4 times during each trial, and consequently 20 times in all. The instructions to the *Os* were those used in Part 1. Although these instructions called for both relative and absolute judgments (*vide supra*), only the absolute judgments were used in the computation of results. The results are stated in terms of Percentages of Pleasantness defined as follows:

a) The Percentage of Pleasantness, P, of a stimulus for a given experimental series in which it has elicited p judgments "pleasant," i judgments "indifferent," and u judgments "unpleasant" is

$$P = \frac{p + i/2}{p + i + u} \cdot 100.$$

b) The Percentage of Pleasantness of a set of stimuli for a given experimental series is the average of the Percentages of Pleasantness of the stimuli in the set for that experimental series.

Part 3 was the same as Part 1, except that the stimuli used in the case of each observer were those which belonged for that observer in Partial Set P (*i.e.* were most pleasant).

Part 4 was identical with Part 2.

In Fig. 2 below is indicated for *Ka* the chronological distribution of the different parts of this experiment. The distribution for the two other *Os* was practically the same as that for *Ka*.

	Part 1. (U Det. Ser.)			Part 2. (Test Ser.)					Part 3. (P Det. Ser.)					Part 4. (Test Ser.)				
Trials:	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T
Days:	0		10		20			30	40			50		60				

FIG. 2. *O* = *Ka*

Results. In Table I are given the Percentages of Pleasantness of the Complete Set as determined in Parts 2 and 4 for each *O*.

TABLE I

O	Part 2 (After Unpleasant Determination)	Part 4 (After Pleasant Determination)
Bu	82%	73%
De	33%	14%
Ka	65%	38%

Inspection of this table shows that for all *O*s the Percentage of Pleasantness of the Complete Set of 21 stimuli was greater in Part 2 of the experiment after determination with the relatively unpleasant stimuli in Partial Set U than in Part 4 of the experiment after determination with the relatively pleasant stimuli in Partial Set P.

Figs. 3–5 indicate for the three *O*s the Percentages of Pleasantness of each of the 21 stimuli of the Complete Set in Parts 2 and 4. The abscissae represent the rank order of the stimuli as determined by their respective Percentages of Pleasantness in Part 2.[3] The ordinates represent the Percentages of Pleasantness of the individual stimuli. The values of the Percentages of Pleasantness determined in Part 2 are indicated by a continuous line; those determined in Part 4, by a broken line.

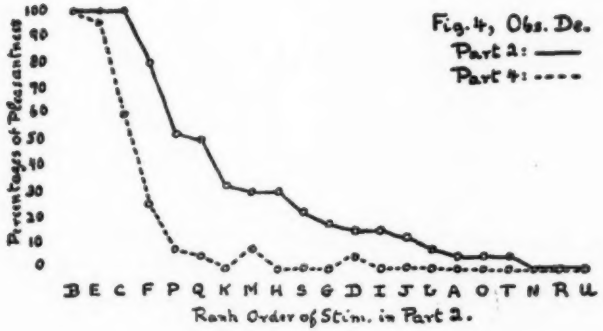
Inspection of Figs. 4 and 5 shows that the change in Percentage of Pleasantness of the Complete Set of stimuli was evenly distributed over very nearly all stimuli, *i.e.* that this change did not consist of a large shift in the affective value of a few stimuli, but in a shift of the affective value of the set as a whole.

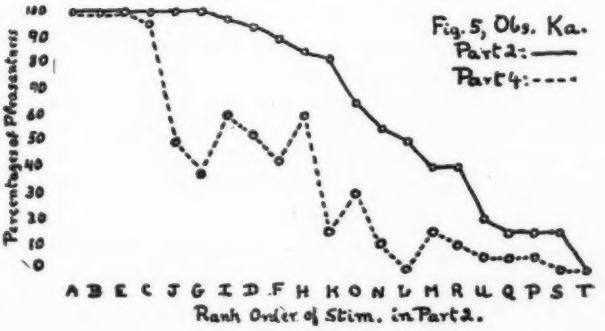
Fig. 3 shows that in the case of *Bu* the changes in the Percentage of Pleasantness of the individual stimuli from Part 2 to Part 4 were decreases in the case of 6 stimuli and increases in the case of 4. This apparent inconsistency with the results for the two other *O*s is adequately accounted for, however, by the slight difference in the case of this *O* between the Percentages of Pleasantness of the Complete Set in Parts 2 and 4, and the consequent probability that uncontrolled variability would obscure results with respect to individual stimuli.

Interpretation of results. The results stated above may be construed as consequences of either of two hypotheses.

[3]Stimuli of which the Percentage of Pleasantness, as determined in Part 2, were the same, were ranked according to their Percentages of Pleasantness as determined in Part 4, or, failing any distinction on this score, according to alphabetical order.

Percentages of Pleasantness
100
90
80
70
60
50
40
30
20
10
0
Fig. 3, Obs. Bu.
Part 2: ——
Part 4: ---
B C D F G J R T A H E K M N O P I L S Q U
Rank Order of Stim. in Part 2.


Percentages of Pleasantness
100
90
80
70
60
50
40
30
20
10
0
Fig. 4, Obs. De.
Part 2: ——
Part 4: ----
B E C F P Q K M H S G D I J L A O T N R U
Rank Order of Stim. in Part 2.


Percentages of Pleasantness
100
90
80
70
60
50
40
30
20
10
0
Fig. 5, Obs. Ka.
Part 2: ——
Part 4: ----
A B E C J G I D F H K O N L M R U Q P S T
Rank Order of Stim. in Part 2.

FIGS. 3–5

The first hypothesis is that the law of affective contrast, established experimentally by Bacon, Rood and Washburn,[4] holds not only in the case of successive presentations separated by brief intervals of time, but also in the case of successive presentations separated by intervals as great as 2 to 10 days. The consequences of this hypothesis in the case of an experiment similar to the one under discussion, but involving only five stimuli, are schematized in Fig. 6.

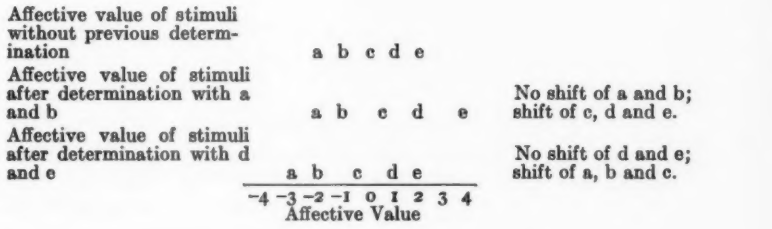

Affective value of stimuli without previous determination
a b c d e
Affective value of stimuli after determination with a and b
a b c d e
No shift of a and b; shift of c, d and e.
Affective value of stimuli after determination with d and e
a b c d e
No shift of d and e; shift of a, b and c.
−4 −3 −2 −1 0 1 2 3 4
Affective Value

FIG 6

The second hypothesis is that the Tendency toward Dynamic Equilibrium, a principle common to Physics and Biology alike,[5] is operative in the field of affection: *the affective value of the experiential correlate of a stimulus varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group.* According to this hypothesis the difference in the affective values of the stimuli in the two Test Series would be due to the fact that in the two cases the experiential correlates of these stimuli were members of different unitary temporal groups, in one of which they were preceded by the unpleasant Determining Series (Part 1 of the experiment), and consequently tended to compensate the predominantly unpleasant affective values of the latter, whereas in the other they were preceded by the experiences of the pleasant Determining Series (Part 3 of the experiment), and consequently tended to compensate a predominance of pleasantness. The consequences of

[4] M. M. Bacon, E. A. Rood, and M. F. Washburn, A study of affective contrast, this JOURNAL, 25, 1914, 290-293.

[5] Cf. L. J. Henderson, *The Order of Nature*, 1925, 51; also K. Lewin, *Vorsatz, Wille und Bedurfnis*, 1926, 33.

this hypothesis in the case of a simplified experiment similar to the one under discussion are indicated in Fig. 7 below.

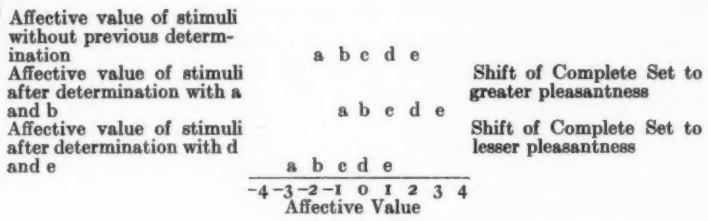

Affective value of stimuli without previous determination
a b c d e
Affective value of stimuli after determination with a and b
Shift of Complete Set to greater pleasantness
a b c d e
Affective value of stimuli after determination with d and e
Shift of Complete Set to lesser pleasantness
a b c d e
-4 -3 -2 -1 0 1 2 3 4
Affective Value

FIG. 7

To determine which of these hypotheses is correct it would be necessary to compare the affective values of the stimuli in the Complete Set, as determined in either of the Test Series, with the affective values of these stimuli as determined in a set of observations which had not been preceded by the presentation of a Determining Series. If in such a comparison it were found that changes in affective value occurred only in the case of those stimuli which had not been used in the Determining Series (cf. Fig. 6), it could be concluded that the first hypothesis (that involving an extension of the law of contrast) is correct, for this hypothesis implies a change in the affective value of a stimulus only when it follows an experience with an affective value different from its own,[6] whereas the second hypothesis is not subject to such a limitation. If, on the other hand, it turned out that there were changes in the affective value of all the stimuli, including those used in the Determining Series, it could be concluded on the same grounds as above that the second hypothesis (that involving the Tendency toward Dynamic Equilibrium) is correct (cf. Fig. 7).

The data of the present experiment do not allow such a comparison, as they yield no information on the Percentages of Pleasantness of the stimuli in the Complete Set in the absence of

[6]Lehmann states the law of affective contrast as follows: "Das von einem gegebenen Reize erregte Gefühl ist auf die Weise von den vorhergehenden Verhältnissen abhängig, dass das Gefühl entweder schwächer wird oder in seinen Gegensatz umschlägt, wenn ein stärkeres Gefühl ähnlicher Art vorausgegangen ist." (A. Lehmann, *Die Hauptgesetze des menschlichen Gefühlslebens*, 2nd. ed., 1914, 251).

previous conditioning with a Determining Series. In order to secure data which would render such a comparison possible, and in order to make sure that the results of the experiment discussed above were not due to a general tendency for olfactory stimuli to become less pleasant with repetition, the writer carried out a further experiment.

II. Distribution of Changes in Affective Value of a Set of Stimuli as a Function of Repeated Presentation of a Part of the Set

Purpose. The experiment described in the present section was performed in order to ascertain whether the change in affective value of a set of stimuli determined by repeated presentation of certain extreme members of this set is distributed evenly over all the members of the set, or whether it is confined to members other than those the repeated presentation of which determined the change.

Technique. The stimuli and general method were the same as in the experiment discussed in the preceding section. The *O*s, three in number, were Messrs. R. A. McFarland (*Mc*), J. Volkmann (*Vo*), and K. E. Zener (*Ze*).

The experiment consisted of three parts. Parts 1 and 3 were Test Series; Part 2 was a Determining Series, *i.e.* an experimental series carried out for the sole purpose of determining changes in the affective judgments of the *O*s.

In Part 1 the Percentages of Pleasantness of the 21 stimuli in the Complete Set were determined for each *O* in a series of observations involving 5 trials distributed over 11 to 12 days. Each trial lasted one hour. Each stimulus was presented 4 times during each trial, and consequently 20 times in all.

In Part 2 there were presented to each *O* in 5 one-hour trials the 10 stimuli which had proved most unpleasant to him in Part 1. Each stimulus was presented 9 times in each trial, and consequently 45 times in all.

In Part 3 the procedure was exactly the same as it had been in Part 1.

The time relations of Parts 1, 2, and 3 are indicated for *Mc* in Fig. 8. The time relations in the case of *Ze* were practically the same. *Vo* was prevented by illness from participating in the full number of trials. In his case the number of trials in each of Parts 1, 2 and 3 was reduced from 5 to 3, and the intervals between these Parts were increased to 18 days between Parts 1 and 2, and 4 days between Parts 2 and 3.

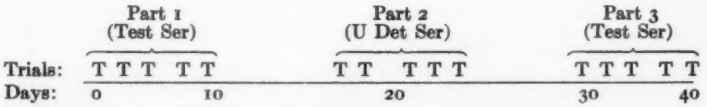


Fig. 8. *O* = *Mc*

Results. In Table II are given the Percentages of Pleasantness of the Complete Set as determined in Parts 1 and 3 for each *O*.

TABLE II

Observer	Part 1 (No previous Determination)	Part 3 (Previous Determination with U stimuli)
Mc	32%	44%
Vo	53%	58%
Ze	49%	58%

The table shows that in the case of all the *O*s the Percentage of Pleasantness of the Complete Set was greater in Part 3 after determination by repeated presentation of 10 unpleasant odors than it had been in Part 1. This fact eliminates the possibility mentioned at the conclusion of Section 1 above, that the results of that section were conditioned not by the presentation of Determining Series, but by a general tendency of olfactory stimuli to become less pleasant as a function of repeated presentation.

Figs. 9–11 indicate for the three *O*s the Percentages of Pleasantness of each of the 21 stimuli of the Complete Set in Parts 1 and 3. The abscissae represent the rank order of the stimuli as determined by their respective Percentages of Pleasantness in Part 1.[7] The ordinates represent the Percentages of Pleasantness of the individual stimuli. The values of the Percentages of Pleasantness determined in Part 1 are indicated by a continuous line, those determined in Part 3 by a broken line.

Although the curves corresponding to Part 3 of the experiment are very jagged in Figs. 9–11,[8] they show clearly that the increase in the Percentage of Pleasantness of the Complete Set, as a function of previous conditioning with a Determining Series comprising the 10 most unpleasant members of this set, involves the stimuli comprised in the Determining Series to roughly the same extent as the other stimuli of the Complete Set.

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[7]Stimuli, the Percentages of Pleasantness of which, as determined in Part 1, were the same, were ranked according to their Percentages of Pleasantness as determined in Part 3, or, in the absence of any distinction on this score, according to alphabetical order.

[8]The extreme irregularity of this curve in the case of *Vo* (Fig. 10) is undoubtedly due to the small number of cases available in the computation of the Percentage of Pleasantness of each stimulus. This circumstance resulted from illness of the *O*.

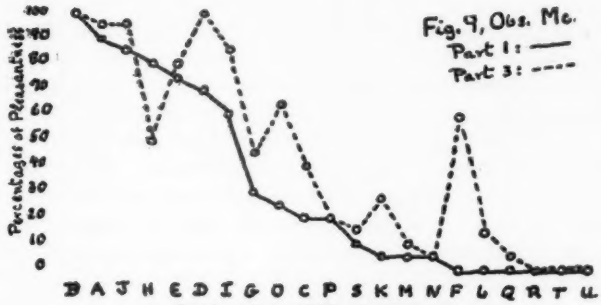
Fig. 9, Obs. Mo.
Part 1: ——
Part 3: - - - -
Percentages of Pleasantness
100
90
80
70
60
50
40
30
20
10
0
B A J H E D I G O C P S K M N F L Q R T U
Rank order of Stim. in Part 1.

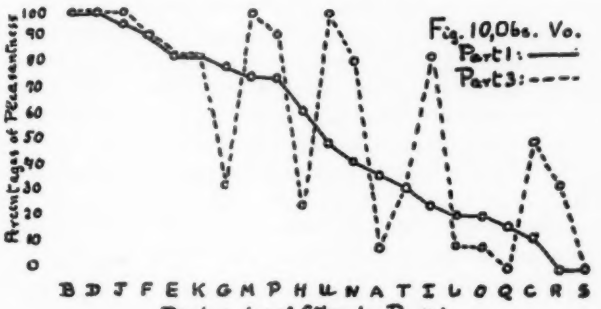
Fig. 10, Obs. Vo.
Part 1: ——
Part 3: - - - -
Percentages of Pleasantness
100
90
80
70
60
50
40
30
20
10
0
B D J F E K G M P H L N A T I L O Q C R S
Rank order of Stim. in Part 1

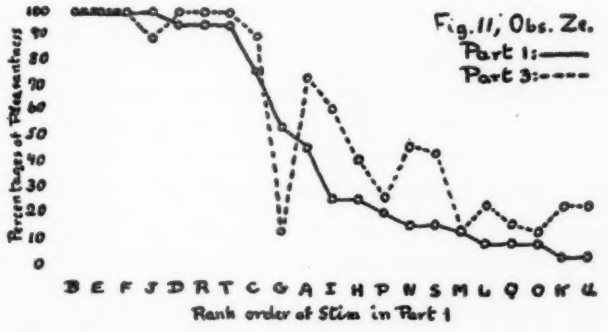
Fig. 11, Obs. Ze.
Part 1: ——
Part 3: - - - -
Percentages of Pleasantness
100
90
80
70
60
50
40
30
20
10
0
B E F J D R T C G A I H P N S M L Q O K U
Rank order of Stim. in Part 1

FIGS. 9–11

Interpretation of results. It was stated at the close of the preceding section that changes in the total affective value of a set of stimuli, as a function of previous determination with certain extreme members of this set, could be construed as consequences of either of two hypotheses. The first of these hypotheses was that the law of affective contrast holds not only in the case of successive presentations separated by brief intervals of time, but also in the case of successive presentations separated by intervals as great as 2 to 10 days. The second hypothesis was that the affective value of the experiential concomitant of a stimulus varies conversely with the sum of the affective values of those experiences preceding it which constitute with it a unitary temporal group.

It was further pointed out that the implications of these two hypotheses were different with respect to the distribution of changes in the total affective value of a set of stimuli over the various members of the set. The first hypothesis implies, namely, that these changes involve only those members of the set which have not been used for purposes of determination, whereas the second hypothesis implies that these changes involve all of the members of the set.

The experiment discussed in the present section has shown that the changes in total affective value of a set of stimuli under the conditions mentioned above involve all of the stimuli in the set, including those used as determining stimuli. These results infirm the first hypothesis and confirm the second. It is concluded, therefore, that affection conforms to the following *Gestaltgesetz*, termed by the writer the law of affective equilibrium: *the affective value of the experiential correlate of a stimulus varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group.*

III. Discussion of the Law of Affective Equilibrium

Facts which can be subsumed under the law. A number of hitherto unrelated facts can, I believe, be considered as consequences of the law of affective equilibrium. A first such fact is the relativity of so-called 'absolute' affective judgments. So obvious is this relativity that Titchener uses, for the method involving such judgments, the name Serial Method, and warns the

reader that "if the method is to be valid, *O* must keep in mind the serial nature of the impressions . . . the affective curve in this case is no more absolute than are the curves of the preceding experiment (by the Method of Paired Comparisons)."[9] Not only does the law of affective equilibrium imply such a relativity; it further implies the particular form which this relativity will take.

A second fact which can be considered as a consequence of the law under discussion is that almost all sensory modalities—indeed the only possible exception seems to be pain—involve both pleasant and unpleasant qualities.[10] This fact, so familiar as usually to escape mention, clearly follows from the law of affective equilibrium in view of the introspective unitariness of modalities.[11]

A third fact which can be subsumed under the law of affective equilibrium is the circumstance that experiments with absolute affective judgments never seem to involve pleasantness alone or unpleasantness alone, but both these qualities in comparable proportions.[12] G. B. Phelan, for instance, in an experiment involving a very large number of stimuli belonging to several modalities, found that, of the total number of feelings reported, 55% were pleasant and 45% were unpleasant.[13] Phelan attributes this distribution to the selection of stimuli: "These percentages," he writes, "do not express a general rule; it is merely a situation of fact. The different frequencies evidently depend upon the

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[9]E. B. Titchener, *Experimental Psychology*, I, pt. ii, 190, 157.

[10]"Every *O*, except *A*, reported both P and U sensations from every sense department. *A* reported them with sensations of smell, taste, and touch. . . . *A* left the University before the completion of her series, and had no opportunity to observe with visual and auditory stimuli." (J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 540).

[11]"Introspection leaves no doubt that the sensations, regarded as qualitative processes, fall into a number of separate groups. All colour sensations, for instance, go together; all tonal sensations go together." (Titchener, *Textbook of Psychology*, 1910, 55).

[12]In comparable, but not in equal proportions. J. C. Flügel has shown that when *O*s record their feelings over long periods of time there is a noticeable predominance of pleasantness over unpleasantness. (Cf. J. C. Flügel, *Brit. J. Psychol.*, 15, 1925, 327 ff.). These results are in harmony with those of a large number of other experiments. Cf. the findings of G. B. Phelan mentioned in the text above; cf. also A. E. Findley, this JOURNAL, 35, 1924, 444, and C. W. Valentine, *Brit. J. Psychol.*, 6, 1913, 190. I suggest that this preponderance of pleasantness may be ascribed to the effects of tradition and learning, which eliminate situations conducive to unpleasantness (*e.g.* extremes of temperature, lack of food, putrid odors) and bring about situations conducive to pleasantness (*e.g.* physiologically favorable temperature, regular meals, perfumes).

[13]G. B. Phelan, *Feeling Experience and its Modalities*, 1925, 112.

stimuli chosen for the experiments."[14] Although I should never venture to question the importance of the stimulus as a partial determinant of affection, I am inclined to see in results such as these the operation not only of the stimulus, but of the law of affective equilibrium.

A fourth fact which can be considered as a special case of the law of affective equilibrium is affective habituation.[15] Let us assume that a single affective stimulus be presented to an observer repeatedly, and that these presentations determine a series of experiences constituting a unitary temporal group. According to the law of affective equilibrium, the second experience of such a series will tend to have an affective value such as to compensate the affective value of the first, the third an affective value such as to compensate the combined affective values of both the first and the second, and the nth experience an affective value such as to compensate the combined affective values of all the $n-1$ preceding experiences. With a sufficient number of repetitions the total affective value to be compensated will increase to such a degree that the affective value of the stimulus will be brought down to indifference. Thus the law of affective equilibrium may be considered to imply affective habituation.

It may be objected that this last subsumption unduly limits the scope of affective habituation by making it contingent upon the unitary grouping of the experiences determined by the repeated stimulus. True, the assertion of this particular contingence requires experimental verification. The assertion of some form of contingence, however, is not only warranted by available experimental data, but required by them. In an experiment by Max Meyer[16] the aesthetic value of quarter-tone music was found to increase, not to decrease, as a function of repetition. A. R. Gilliland and H. T. Moore found enjoyment of musical selections to increase with repetition.[17] The aesthetic enjoyment of pictures also increased as a function of repetition in an experiment by L. J.

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[14] *Ibid.*, 113.

[15] Sometimes called affective adaptation. The term habituation is here used to distinguish adaptation to repeated stimulation from the more common form of adaptation, namely to continuous stimulation.

[16] M. Meyer, Experimental studies in the psychology of music, this JOURNAL, 14, 1903, 207-214.

[17] A. R. Gilliland and H. T. Moore, The immediate and long time effects of classical and popular phonograph selections, *J. Appl. Psychol.*, 8, 1924, 309-323.

Martin.[18] Mere repetition of a stimulus, then, cannot be considered an unconditional determinant of affective habituation.

Equivocality of the law. So much for the merits of the law of affective equilibrium. A few words now upon an obvious defect in this law, namely its equivocality. It must be recognized that the entities subject to the law are ill-defined. What enables one to decide whether a given set of affective experiences does, or does not, constitute a unitary temporal group? The experiments reported above clearly show that a series of odors experienced under the same general conditions—in the same room, at the same hour of the day, in the presence of the same experimenter and under the same instructions—does indeed constitute such a unitary group, and it seems safe to infer that any series of experiences belonging to a single modality and presented under the same general conditions would likewise constitute a unitary group. But what of series involving experiences of different modalities—of series involving both visual and auditory experiences, for instance? Would these series also constitute unitary groups to the extent required by the law of affective equilibrium? And what of such complex events as 'arising in the morning' or 'eating a good meal' or 'enjoying a book'—could these, likewise, constitute unitary groups such that their successive members would tend affectively to compensate the preceding ones? What subjective factors, finally, play a part in establishing unitary temporal groups of affective experiences? Would it be possible to exert some control over one's affective life by the conscious grouping of various experiences?

Until such questions as these have been answered, the law of affective equilibrium can make little pretense to univocality. Fortunately these questions lend themselves to experimental investigation, and consequently the present shortcomings of the law with respect to univocality, far from being permanent defects, constitute essentially just so many invitations to further research.

Lack of satisfactory explanation. A perceptual law somewhat similar to the law of affective equilibrium has been ascribed by Wertheimer to the operation of a 'set' which he calls "spatial

[18] L. J. Martin, An experimental study of Fechner's principles of aesthetics, *Psychol. Rev.*, 13, 1906, 175-179.

level."[19] By adapting Wertheimer's conception of 'level' to the needs of the case it would be possible to consider the law of affective equilibrium as the manifestation of an 'affective level.' But would this really be an explanation? I think not. A 'set,' it seems to me, is either an *ad hoc* explanatory concept, an X whose entire nature consists of causing the events described by some specific law, or it is a real entity with definite spatio-temporal and qualitative characteristics. If it be the former, it is nothing but an intellectual anaesthetic. If the latter, its use is legitimate only on condition that both its spatio-temporal characteristics and its qualitative characteristics be defined at least in some degree. Merely to state that it is a physiological event strikes me as a superfluous confession of faith. As I am unable at present to imagine what would be the spatio-temporal characteristics of an 'affective level,' I feel that the use of such a 'set' as an explanation of the law of affective equilibrium would bring about merely a pseudo-solution, harmful to the future of the problem.

As to some other and more satisfactory type of explanation, none has so far suggested itself to me.

Summary

The present paper describes two experiments upon changes in the affective value of a set of stimuli as a function of frequent presentation of affectively extreme members of the set. The first experiment shows that the affective value of a set of olfactory stimuli, as determined 2 or more days after frequent presentation of the 10 most unpleasant members of this set, is greater than the affective value of the set as determined 2 or more days after frequent presentation of the 10 most pleasant members of the set. The second experiment shows that the changes in affective value demonstrated in the first experiment involve the entire set of stimuli, including those members whose frequent presentation have determined these changes.

From these facts it is concluded that affection conforms to the following general law, termed the law of affective equilibrium: *the affective value of the experiential correlate of a stimulus*

[19]Cf. Max Wertheimer, *Drei Abhandlungen zur Gestalttheorie*, 1925, 100.

varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group.

In a brief discussion of this law it is pointed out that it may be construed to imply a number of hitherto unrelated phenomena, namely the relativity of so-called 'absolute' affective judgments, the affective ambivalence of sensory modalities, the occurrence of both pleasant and unpleasant experiences in almost all experiments upon affection, and the fact of affective habituation. The discussion further involves the admission that in its present formulation the law is somewhat equivocal, but this deficiency is shown to be one which can be remedied by further experimentation. The problem of explaining this law is briefly dealt with at the close of the discussion, but left unsolved.

FITTING OF FREQUENCY FUNCTIONS TO URBAN'S LIFTED-WEIGHT RESULTS

By GODFREY H. THOMSON, University of Edinburgh

I. Statement of the Tail-Difficulty
II. Various Tail-Assumptions
III. Test for the Sufficiency of the Simple Tail-Assumption
IV. Rules for the Bipartite Gaussian Tail-Assumption
V. Sample Calculation of β_1, β_2, and κ_2
VI. Probable Errors of the Frequency Constants by Pearson's Formulae
VII. Theoretical Values Calculated from the Constants of Tables III and IV

The paper by H. M. Lufkin on "The Best Fitting Frequency Function for Urban's Lifted-Weight Results,"[1] has reminded me of a manuscript that I prepared on that subject in 1914 which, after lying in the possession of Professor Titchener during the war, was in 1919 awarded the prize which Urban had offered for such an essay.[2] Urban's continued residence in Europe, and the great bulk of the MS, no doubt explain how it came to be unpublished even in part. When visiting America in 1923–24 I stayed for a few days with Professor Titchener, who then returned the MS to me. Part of the MS had meanwhile supplied material for a chapter in a book on *The Essentials of Measurement*,[3] where one or two of the curves I had obtained are illustrated by diagrams.

Lufkin's table giving the Pearsonian Curve constants for Urban's lifted-weight data may be very misleading to readers who lack the time to delve into the necessary mathematics and arithmetic, because he omits to mention a fundamental difficulty in fitting Pearsonian curves to these data, a difficulty which I have called the "tail-assumption" difficulty, arising at bottom from the fact (a) that the Pearsonian equations are not in general integrable, and (b) that the Urban lifted-weight experiments did not in the case of any S extend from a point where every answer was 'heavier' to a point where every answer was 'lighter.'[4] This tail-difficulty is not referred to by Lufkin, and it is not possible from his paper to tell what tail assumption he wittingly or unwittingly made, nor can one check his figures. I have been unable to obtain the constants which he gives on any tail-assumption that I can think of.

In most of what follows I quote direct from the Urban prize essay referred to above.

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*Accepted for publication May 3, 1928.

[1] This JOURNAL, 40, 1928, 75-82.

[2] For announcements concerning this prize see this JOURNAL, 25, 1914, 148; 26, 1915, 620.

[3] W. Brown and G. H. Thomson, Chap. IV. Skewness and heterogeneity in psychophysical data, *op. cit.*, 1921.

[4] Cf. *ibid.*, 52, 62 f., 75, 89, 92.

It will be remembered that the 'heavier' and 'lighter' data of Urban's experiments form ogives, and that the idea of fitting Pearsonian curves involves fitting not the data themselves but the differences between them. This is explained at some length in *The Essentials of Measurement* where the answers 'heavier' or Urban's *S* I are used in illustration.[5]

I. Statement of the Tail-Difficulty

At the very outset of the work we are met by a difficulty which will be found to dominate the whole, a difficulty caused by the fact that the tails of the frequency diagram are incompletely known. Consider for example the case of the answers 'heavier' given by *S* I. Here we know that 8 thresholds are between 84 and 88 grm., 31 between 88 and 92 grm., and so on till we have accounted for 422 out of 450 thresholds. Of the remainder we only know that 27 lie somewhere above 108 grm. and one somewhere below 84 grm. We can handle the zero moment because we know the total area, but before we can find any higher moments we must make some assumption as to the distribution of these tails. For instance suppose we wish to find the first moment or, to put the same thing in another way, suppose we wish to find the arithmetical mean of these 450 thresholds, a central value which has been recommended by Spearman[6] and later by Wirth.[7] To obtain this we must assume values for the abscissae of the points at which the tails are to be concentrated; and since this will be at best a very arbitrary proceeding it seems hopeless to base thereon complicated calculations such as those necessary in employing Pearson's formulae.

It is true that this difficulty will not arise in experiments wherein the series of stimuli is so extended that these tails are completely known, that is to say series in which *S always* answers 'heavier' at the highest weight and *never* answers 'heavier' at the lowest weight. All who have carried out such experiments, however, know how difficult it is to fulfil this condition, and indeed in one kind of experiment it is impossible, namely in experiments on an absolute threshold such as the spatial threshold. The following is an example of data of such experiments, where 150 observations were taken at each stimulus value:

Cm.	0	0.5	1	1.5	2	2.5	3	3.5	4	4.5	5
Answers "Two"	5	6	8	21	56	84	105	125	141	144	148

There are five thresholds *below zero*, and it is clearly impossible to extend the stimulus series in this direction.

It will be of course be quite wrong simply to ignore such extreme values. This is precisely on a par with setting out to find the average height of Englishmen but refusing to measure any men above 6 ft. or below 5 ft. 6 in. The

[5] See p. 61 (bottom half), and diagrams on pp. 62, 90.

[6] C. Spearman, The method of 'right and wrong cases' ('constant stimuli') without Gauss's formulae, *Brit. J. Psychol.*, 2, 1908, 227.

[7] W. Wirth, *Psychophysik: Darstellung der Methoden der experimentellen Psychologie*, 1912, 122.

arithmetic mean which we need in order to apply Pearson's methods is the centroid of the whole bell curve, not of a portion of it, and similarly the higher moments must refer to the whole bell curve. The ultimate reason for this is that Pearson's curves are not generally integrable but only between certain limits. In general they reduce to an Eulerian integral.

A tail-assumption is avoidable if the curve we choose is integrable. If we wish, as we do, to use Pearson's curves we cannot integrate the equations which give the defined area and the first moment. We turn therefore to the question of what assumption we are going to make as to the distribution of the tails.

II. Various Tail-Assumptions

The simplest thing to do is to assume that at 80 grm. *S* I would never, and at 112 grm. would always, have answered 'heavier,' *i.e.* to center the tails at 82 and 110 grm. respectively. This is done by way of example in *The Essentials of Measurement.*[8] The calculation is continued,[9] the values $\beta_1 = 0.251$, $\beta_2 = 3.510$, and $\kappa_2 = 0.775$ are arrived at, and the conclusion is drawn that the type is Pearson's Type IV, though within the limits of probable error of β_1, β_2 and κ_2 it might be Type G, V or VII.[10]

Not only, however, are the constants β_1, β_2 and κ_2 liable to the usual probable errors of sampling. It is also clear that the whole calculation and the type of curve found depends very largely on our tail-assumption. This can be perhaps most forcibly shown by assuming that the data, instead of stopping at 84 grm. and 108 grm., had gone on in the following, not at all unlikely, fashion.

Grm.	80	84	88	92	96	100	104	108	112	116	120
Answers 'heavier'	0	1	9	40	100	186	403	423	439	446	450

If this were so we should, on carrying out similar calculations to the above find, instead of the asymmetrical Type IV, the symmetrical Type VII, with the values found for the constants being $\beta_1 = 0.000$, $\beta_2 = 4.363$, and $\kappa_2 = 0.000$. We need under these circumstances some tail-assumption which will be (1) easy to apply, (2) probable in itself, (3) yielding to the present asymmetry of the data, and (4) capable of being defined so that arbitrary personal bias cannot enter in its application. For this purpose the writer has chosen after many trials a bipartite gaussian tail-assumption, that is one which extends the data to right and left by two gauss curves, differing if necessary at each end. A middle point of the data is chosen (that nearest to 0.5 for the frequency ratio) and one gauss curve drawn through this and the top point, another through this and the bottom point. The tails are used to extend the data. The bipartite gaussian method strikes a mean between the simple assumption which exaggerates asymmetry and the ordinary (single) gaussian which exaggerates symmetry. Bipartite gaussian tails are given for all the *S*s in Tables I and II.

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[8] Brown and Thomson, *op. cit.*, 63 ff. for *S* I 'heavier.'
[9] *Ibid.*, 87 f.
[10] The Type IV curve is drawn on p. 90.

TABLE I

THE NUMBER OF ANSWERS 'LIGHTER' AT 84–108 GRM., WITH THE CONTINUATIONS CALCULATED BY THE BIPARTITE GAUSSIAN TAIL-ASSUMPTION

	Grams													
S	Tail			Actual Data							Tail			
	72	76	80	84	88	92	96	100	104	108	112	116	120	124
I	450	449	443	421	385	261	151	55	6	3	0			
II	450	448	441	420	388	315	202	104	43	7	1	0		
III			450	448	430	391	315	171	27	12	1	0		
IV		300	297	288	277	230	132	63	15	6	1	0		
V		300	297	288	278	210	87	53	10	6	2	0		
VI		300	299	292	259	215	147	62	20	6	1	0		
VII		300	299	294	269	222	137	80	18	12	3	1	0	

TABLE II

THE NUMBER OF ANSWERS 'HEAVIER' AT 84–108 GRM., WITH THE CONTINUATIONS CALCULATED BY THE BIPARTITE GAUSSIAN TAIL-ASSUMPTION

	Grams													
S	Tail			Actual Data							Tail			
	72	76	80	84	88	92	96	100	104	108	112	116	120	124
I			0	1	9	40	100	186	403	423	447	450		
II		0	2	10	11	50	132	238	367	407	438	448	450	
III				0	11	32	100	212	402	431	448	450		
IV		0	2	7	8	35	107	183	265	279	294	299	300	
V		0	1	8	7	48	154	204	261	283	295	299	300	
VI			0	2	7	19	56	126	223	265	291	299	300	
VII			0	2	2	20	51	131	227	246	278	293	299	300

No doubt the best method of all would be to find a curve which passed through the top and bottom points and whose course was influenced by all the other points. This could be done in various ways but the arithmetic and work would be too tedious for a mere preliminary approximation.

When to the data thus extended by the bipartite gaussian tail assumption the necessary calculations are applied, the Pearsonian constants then found are as in Tables III and IV.

In both Tables III and IV:

(1) All calculations were carried out to seven significant figures although for economy of space only five are given in these summaries. Probably only five are reliable anyhow.

(2) The sign of the skewness is consistent with the ordinary convention that from 84 grm. towards 108 grm. is *positive*. In actual working, however, while this convention was used for the 'lighter' curves, the opposite convention was used for the 'heavier' curves, so that the actual data formed the first Hardy summation in each case. Hence the sign of μ_3 is in the former the same, in the latter not the same, as the sign of skewness.

These constants differ in every instance from those given by Lufkin, which I have not been able to obtain. The whole illustrates the fact that to the ordinary probable errors of the constants (in any case doubtful since the data

TABLE III

PEARSONIAN CONSTANTS FOR THE ANSWERS 'LIGHTER'

S	Mean	μ_2	μ_3	μ_4	β_1	β_2	κ_2	Mode	Approx. Median	Skewness	Strict Type	Types Calculated
I	93.324	2.0081	0.6109	12.730	0.04608	3.1568	+0.200	93.900	93.516	−.1016	±.039	IV	IV,G,III, Compounds
II	95.057	2.8223	1.2724	24.208	0.07202	3.0391	−0.399	96.003	95.373	−.1406	±.038	I	I,G, Charlier
III	97.955	1.5721	0.7827	8.5019	0.15766	3.4399	0.302	98.840	98.250	−.1762	±.038	IV	IV,G.
IV	95.453	2.0080	0.4242	15.228	0.02222	3.7767	0.011	—	—	−.0518	±.051	IV	VII,G.
V	94.413	1.8827	0.7527	15.427	0.08489	4.3524	0.027	—	—	+.0874	±).088	IV	VII,G,Compounds
VI	95.346	2.2667	0.0657	14.285	0.00037	2.7804	−0.000	—	—	−.0070	±.048	I	II,G.
VII	95.8	2.3708	0.9657	19.018	0.06998	3.3835	0.096	95.106	95.569	+.1126	±.049	IV	IV,G.

Here the sign of μ_3 is the same as that of the Skewness.

TABLE IV

PEARSONIAN CONSTANTS FOR THE ANSWERS 'HEAVIER'

S	Mean	μ_2	μ_3	μ_4	β_1	β_2	κ_2	Mode	Approx. Median	Skewness	Strict Type	Types Calculated
I	99.697	1.5876	0.8063	9.1890	0.16248	3.6456	0.158	100.516	99.970	−.1623	±.039	IV.	IV,G,III, Compounds
II	99.306	2.4833	0.3435	22.374	0.00771	3.6283	0.005	—	—	−.0320	±.042	IV.	VII,G.
III	99.457	1.4327	0.6482	7.5189	0.14286	3.6629	0.124	100.171	99.965	−.1491	±.039	IV.	IV,G.
IV	98.28	2.2351	0.0427	20.591	0.00016	4.1218	0.000	—	—	+.0038	±).049	IV.	VII,G.
V	97.2	2.3433	1.3240	19.718	0.13623	3.5908	0.137	96.285	96.895	+.1495	±.047	IV.	IV,G.
VI	100.826	2.1306	0.4598	15.652	0.02186	3.4479	0.020	—	—	−.0588	±.051	IV.	VII,G.
VII	101.346	2.6200	1.7458	23.357	0.16947	3.4026	0.447	100.127	100.940	+.1884	±.047	IV,	IV,G.

Here the sign of μ_3 is the opposite from that of the Skewness. See text.

are *difference* data) must be added the uncertainty of the tail-assumption. To clinch this I assemble here some of the various values I have obtained for β_1 for *S* I, 'heavier,' by different treatment of the tails.

Tail-Assumption	*Sheppard's correction used?*	β_1
The simplest	Yes	0.251
Symmetrical gaussian	Yes	0.083
Bipartite gaussian	Yes	0.162
Tails given above	Yes	0.000
Ignoring tails and taking N = 422	No	0.947
" " " " " " "	Yes	1.167

The last two methods are of course quite indefensible, but I tried them among others in my search for Lufkin's value 1.052.

III. Test for the Sufficiency of the Simplest Tail-Assumption

This simplest tail-assumption, as has been explained, is that at the next step in each direction all, or none, of the answers would be in a certain category. If the experiments really almost reach these points it is clear that the simple tail-assumption is justifiable. This leads to the idea of a test for the data to see whether this is so, as a guide while experimenting, if one wishes to use Pearson's curves.

It is clear that the greater the number of experiments N at each step, the nearer must we go to p = 1 or 0 before we can assume that one more step will bring us either no answers or N answers of the specified kind. We can get a rough and ready test if we assume that our middle-most stimulus is at or near p = 0.5 and if we take N = 100 and N = 1000 as typical values for N. We get then the following ranges over which the *E* must spread his (equidistant) steps if he is to use the Simple Tail-assumption.

No. of stimuli = 1 + No. of steps	Range for N = 100	Range for N = 1000
5	0.060 to 0.940	0.020 to 0.980
6	0.048 to 0.952	0.013 to 0.987
7	0.040 to 0.960	0.010 to 0.990
8	0.035 to 0.965	0.008 to 0.992
9	0.031 to 0.969	0.007 to 0.993
10	0.029 to 0.971	0.006 to 0.994

This is to say an *E* making 1000 experiments at each stimulus and using ten stimuli may use the Simple Tail-assumption only if his *S* has answered 'heavier' (or whatever answer he is considering) not more than six times at the stimulus at one end and not less than 994 times at the other end, or at any rate thereabouts.

For any other value of N an approximate range can be seen from the small table above, or the proper range can be calculated thus: Let *E* look out in Table I of Pearson's Tables the deviate corresponding to a permille of 1/N. Then let him take $n-1/n+1$ of this, where n is the number of stimuli; and for this deviate thus calculated Pearson's Table I will give the range. The reader should first calculate some of the ranges given above in this way.

Judged by this test none of Urban's curves are satisfactory at *both* ends.

IV. Rules for the Bipartite Gaussian Tail-Assumption

It must of course be emphasised that the use of a bipartite gaussian curve to give the tails is only a preliminary step; the Pearson calculation is then carried out on the data thus extended.

Let the data[11] be

	Grams	p
A	84	0.0022
	88	0.0200
	92	0.0889
	96	0.2222
B	100	0.4133
	104	0.8956
C	108	0.9400

In Table I of Pearson's Tables or in some other Table of the Probability Integral find the deviates which correspond to the permilles p at the end points A and C and at the point B where p is nearest to 0.5 (for cases where two points are exactly or nearly equidistant from 0.5 see later). We thus get:

	Grams	p	Deviate	
A	84	0.0022	−2.8521	
	88	0.0200		Difference 2.6330
	92	0.0889		
	96	0.2222		
B	100	0.4133	−0.2191	Difference (algebraic) 1.7739
	104	0.8956		
C	108	0.9400	+1.5548	

The deviate-difference from A to B is 2.6330, and, as this is a distance of four steps, the gauss curve here is proceeding in steps of 2.6330 ÷ 4, that is 0.65825, in terms of the standard deviation. Therefore the deviate for 80 grams will be 2.8521 + 0.65825, that is 3.51035. The permille corresponding to this is less than 0.001, and the number of answers *heavier* therefore less than 0.450, or *zero* to the nearest integer. Thus the extension to the left of the first line of Table II, *supra*, is explained.

Between B and C are two steps, and the deviate-difference is 1.7739. Each step is therefore 0.88695. At 112 grm. the deviate is therefore 1.5548 + 0.88695, that is 2.44175, to which corresponds a permille of 0.9927, and the number of answers 'heavier' is 450 times this, or 447. At 116 grm. the deviate is 3.3287 which corresponds to a permille greater than 0.999 and therefore to 450 answers *heavier* to the nearest unit.

If there be two points B and B^1 nearly equidistant from 0.5 then one gauss integral curve is drawn through A and B ($B<0.5$) and another through B^1 and C ($B^1>0.5$).

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[11] These are again the data of *S* I 'heavier,' here given in decimal form. In most calculations, however, the actual numbers of answers were employed.

V. Sample Calculation of β_1, β_2, and κ_2

Subject I *heavier*.

Grams		Tail-Assumption — Continued sums		Bipartite Gaussian (Hardy's method of finding moments)	
84		1	1	1	1
88		9	10	11	12
92		40	50	61	73
96		100	150	211	284
100		186	336	547	831
104		403	739	1286	2117
108		423	1162	2448	4565
112		447	1609	4057	8622
450 trials	50	1609	4057	8622	16505
=9×50	9	32.18	81.14	172.44	330.1
		3.575	9.015	19.16	36.67
		d	S_3	S_4	S_5

Mean = 114 − 4d = 99.697

3+2d = 10.151

log d = 0.5533435

log 1+d = 0.6604439

1.2137874 — d(1+d) = 16.36015

log 2+d = 0.7462881

1.9600755 — d(1+d)(2+d) = 91.21694

log 3+d = 0.8179324

2.7780079 — d(1+d)(2+d)(3+d) = 599.8019

$2S_3$ = 18.03111

sub. d(1+d) = 16.36015

v_2 = 1.67096

sub. Shepp. = 0.08333

μ_2 = 1.58763

log μ_2 = 0.2007493

log v_2 = 0.2229661
log 3 = 0.4771213 } add
log (1+d) = 0.6604439

log w = 1.3605313
log 2 = 0.3010300 } add
log (2+d) = 0.7462881

log y = 2.4078494

w = 22.93672

d(1+d)(2+d) = 91.21694

114.15366

sub. *from* $6S_4$ = 114.96

μ_3 = +0.80634

(μ_3 may be negative)

log μ_3 = $\bar{1}$.9065182

log (3+2d) = 1.0065135

log 2 = 0.3010300

log μ_3 = $\bar{1}$.9065182

log x = 1.2140617

d(1+d)(2+d)(3+d) = 599.8019

y = 255.7699

x = 16.37049

z = 871.94229

(Note: x is negative if μ_3 is negative)

S_5 = 36.67

6

220.06

4

880.26667

log μ_3^2 = $\bar{1}$.8130364

log μ_2^3 = 0.6022479

log β_1 = $\bar{1}$.2107885

β_1 = 0.1624757

	880.26667	
$\frac{1}{2}\nu_2$	= 0.83548	add
Shepp.	= 0.02916	
	881.13131	
Sub. z	= 871.94229	
μ_4	= 9.18902	
log μ_4	= 0.9632692	
sub log μ_2^2	= 0.4014968	
log β_2	= 0.5617706	
β_2	= 3.645613	

In practice the calculation can be stopped at this point. From β_1 and β_2 with the aid of the diagram on p. 66 of Pearson's Tables the type of the curve can be found without actually proceeding to the calculation of the criterion κ_2. It proves to be Type IV. For explanatory purposes however we need κ_2 and calculate it as follows.

log (β_2+3)	= 0.8225351	$4\beta_2$	= 14.582452
again	0.8225351	$3\beta_1$	= 0.487427
log β_1	= 1.2107885	M	= 14.095025
log Numerator	= 0.8558587	$2\beta_2$	= 7.291226
log. Denom.	= 1.6562732	$3\beta_1$+6	= 6.487427
log κ_2	= 1.1995855	W	= 0.803799
κ_2 = + 0.158381		log 4	= 0.6020600
		log M	= 1.1490657
Sign is same as M W.		log W	= 1.9051475
		log Denom.	= 1.6562732

VI. Probable Errors of the Frequency Constants by Pearson's Formulae

These probable errors have been fully worked out and tabulated by Pearson for the case of ordinary frequency diagrams. There are two complications in our case to which we must return later, namely (1) the inaccuracy introduced by the tail-assumption and (2) the fact that our data are originally in ogive form; but for the present it will be valuable to ignore these difficulties and examine our constants with the aid of Pearson's Tables. Tables V and XLIII in *Tables for Biometricians and Statisticians* give the probable error of κ_2, Tables XXXVII and XXXVIII that of β_1 and β_2, and Table XLI that of the skewness. The figure here presented shows Pearson's Diagram XLVII with the ringed point $\beta_1\beta_2$ marked for Urban's *S* I 'heavier' (with the bipartite gaussian tail-assumption), surrounded by its probability-ellipse, that is a contour of the frequency surface of the variates β_1 and β_2 for N = 450, and such that the volume enclosed is just one half the frequency. The odds are 1 in 2, or evens, that the dot representing our curve could fall (on another sample being made) outside this ellipse. Since the ellipse crosses the lines representing Types III and V, crosses the area representing VI, projects into the area representing I, and is not far from the line representing VII and the point G representing the gauss normal curve, it is perfectly clear that several equations would adequately represent the data, and that the gauss curve itself would not give a *much* more improbable fit than Type IV at the dot. The above

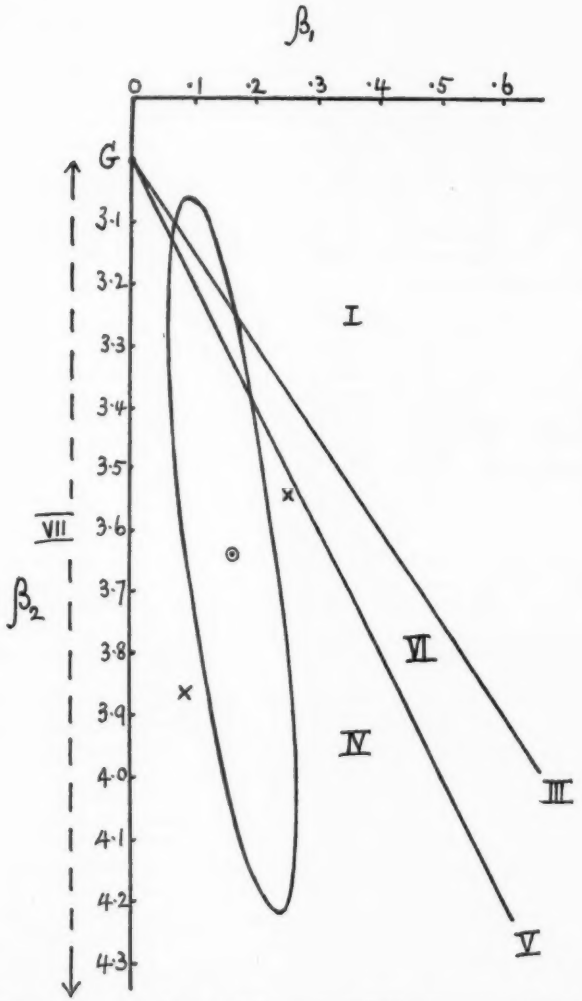

FIG. 1. SHOWING PEARSON'S[3] DIAGRAM XLVII WITH RINGED POINT SURROUNDED BY ITS PROBABILITY-ELLIPSE

probable errors, moreover, are calculated on the assumption that the tails (really assumed by a bipartite gauss curve) are actually known by observation. The fact that this is not the case introduces still further uncertainty into the determination of type. For example the simple tail-assumption, and a symmetrical gaussian tail assumption, give rise to the points marked with crosses on the diagram, each of which of course would be surrounded by its appropriate probability ellipse.

To this double uncertainty must be added yet another point connected with what may be called the Bernoulli weighting of the data. It arises from the fact that what is really observed is not a frequency diagram but points on an ogive.

The data for example of *S* I 'heavier' are in the form of proportional frequencies. Consider the answers at 100 grm. The proportional frequency of answers 'heavier' here was

$$p_{100} = \frac{186}{450} = 0.4133$$

Now if we compare this experiment at 100 grm. with drawing a ball on 450 occasions (replacing it as soon as its color is observed) from a bag containing a very large number of balls of which some are white and some black, we can write down by Bernoulli's Theorem[12] the probable error of the result p = 0.4133. This probable error will be

$$.6745\sqrt{\frac{p\,(1-p)}{N}}$$

where N is the number (450) of trials made.[13] Our only interest here is however this, that the points of the experimental ogive are not all equally certain, although the same number of trials has been made in determining each. Each point has its own weight, and the weighting of the different parts of the frequency diagram which is derived from the experimental ogive is still more complicated.

Of this weighting the Pearsonian method takes no account.[14] It is intended for data already in the form of a frequency diagram, not for data in the form of proportional frequencies composing an ogive. Therefore (and this is our point here) the above probable errors are not the correct ones, for the complications mentioned are not consciously allowed for in any way.

We may sum up our argument of the last few pages. We can find by Pearson's Tables the probable error of the frequency constants for our data. These probable errors are so large as to cause us to doubt whether the gauss normal

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[12]Really this is the inverse use of Bernoulli's Theorem. In any case it ought to be called rather Bayes' Theorem, and the simple formula given in the text does not hold when p is nearly zero or unity.

[13]The use of Bernoulli weights to make allowance for these probable errors was first advised by Urban, and a discussion thereon, with examples drawn from Urban's material, will be found in W. Wirth, *Psychophysik*, 1912, 148-151.

[14]By this is meant that the Pearsonian method does not consciously make allowance for those weights; but it does not, on the other hand, weight all points in the same way.

curve is not good enough; and there are further complications arising from the tail-assumption and the Bernoulli weights. The uncertainty of the tail-assumption will still further increase the probable error, while the effect of the unequal weighting, though small, is to increase our ignorance.

VII. Theoretical Values Calculated from the Constants of Tables III and IV

Using the given constants, the bell curves can be calculated, and from them, by a quadrature formula, the values shown in Table V, where the theoretical values are shown over a range from 80 to 120 grm., and compared with the experimental values from 84 to 108 grm.[15]

TABLE V

Comparison of Theoretical and Experimental Values

S	Grm.	Lighter	Observed	Heavier	Observed	Equal	Observed
	80			0.0011			
	84	0.9444	−.0088	0.0045	−.0023	0.0511	.0111
	88	0.8289	.0267	0.0184	.0016	0.1527	−.0273
	92	0.6057	−.0257	0.0690	.0199	0.3253	.0058
	96	0.3262	.0094	0.2155	.0067	0.4583	−.0161
I	100	0.1145	.0077	0.5006	−.0873	0.3849	.0795
	104	0.0235	−.0102	0.8078	.0878	0.1687	−.0776
	108	0.0025	.0042	0.9624	−.0224	0.0351	.0182
	112			0.9962			
	116			0.9998			
	120			0.9999			
	72	0.9994					
	76	0.9958					
	80	0.9824		0.0017		0.0159	
	84	0.9432	−.0099	0.0092	.0130	0.0476	−.0032
II	88	0.8520	.0102	0.0356	−.0112	0.1124	.0009
	92	0.6875	.0125	0.1152	−.0041	0.1973	−.0084
	96	0.4627	−.0138	0.2895	.0038	0.2478	.0100
	100	0.2379	−.0068	0.5465	−.0176	0.2156	.0244
	104	0.0858	.0098	0.7830	.0326	0.1312	−.0423
	108	0.0187	−.0031	0.9209	−.0165	0.0604	.0196
	112	0.0019		0.9762		0.0219	
	116			0.9936			
	120			0.9983			

[15]Other matters dealt with in the essay were, in addition to the necessary elaborations of the above outline, as follows. Examples of fitting integrable curves without a tail assumption. Discussion of the prevalence of Type IV. Tests of the probable errors by subdivision of the data. Diagrams of the strict curves, G curves, Type III curves, and in one case a Charlier curve. A discussion of the inaccuracy of Bernoulli weights at extreme values. Comparisons of accuracy of the various curves (a) based on Σv^2, (b) based on the Pearson criterion of goodness of fit. An attempt to fit compound curves. On differences in heterogeneity in the different *S*s. An attempt to make a family of ogives which could be fitted direct to the data and not to the differences. Some new tables were finally suggested. The general conclusions may be taken as summed up on p. 94 of Brown and Thomson, *op. cit.*

TABLE V (Concluded)

S	Grm.	Lighter	Observed	Heavier	Observed	Equal	Observed
	80	0.9989		0.0005		0.0006	
	84	0.9929	.0027	0.0032	−.0032	0.0039	.0005
	88	0.9676	−.0120	0.0154	.0090	0.0170	.0030
	92	0.8825	−.0136	0.0647	.0064	0.0528	.0072
III	96	0.6747	.0253	0.2181	.0041	0.1072	−.0294
	100	0.3583	.0217	0.5248	−.0537	0.1169	.0320
	104	0.1043	−.0443	0.8368	.0565	0.0589	−.0122
	108	0.0138	.0129	0.9729	.0151	0.0133	.0023
	112	0.0013		0.9975		0.0012	
	116			0.9998			
	120			0.9999			
	80	0.9943					
	84	0.9765	−.0165	0.0118	.0115	0.0117	.0050
	88	0.9126	.0107	0.0415	−.0148	0.0459	.0041
	92	0.7419	.0248	0.1326	−.0159	0.1255	−.0088
IV	96	0.4588	−.0188	0.3384	.0183	0.2028	.0005
	100	0.1981	.0119	0.6239	−.0139	0.1780	.0020
	104	0.0622	−.0122	0.8465	.0368	0.0913	−.0246
	108	0.0162	.0038	0.9510	−.0210	0.0328	.0172
	112	0.0040					
	116	0.0010					
	120	0.0003					
	76	0.9979					
	80	0.9922		0.0109		−.0031	
	84	0.9703	−.0103	0.0192	.0075	0.0105	.0028
	88	0.8921	.0346	0.0629	−.0396	0.0450	.0050
	92	0.6863	.0137	0.1990	−.0390	0.1147	.0253
	96	0.3742	−.0842	0.4401	.0732	0.1857	.0110
V	100	0.1377	.0390	0.6919	−.0119	0.1704	−.0271
	104	0.0392	−.0059	0.8605	.0095	0.1003	−.0036
	108	0.0103	.0097	0.9415	.0018	0.0482	−.0115
	112	0.0028		0.9724		0.0248	
	116	0.0008		0.9825		0.0167	
	120	0.0002		0.9856		0.0142	
	80	0.9936		0.0007		0.0057	
	84	0.9687	.0046	0.0035	.0032	0.0278	−.0078
	88	0.8853	−.0220	0.0159	.0074	0.0988	.0145
	92	0.7058	.0109	0.0630	.0003	0.2312	−.0112
VI	96	0.4582	.0318	0.1957	−.0090	0.3461	−.0228
	100	0.2255	−.0188	0.4410	−.0210	0.3335	.0398
	104	0.0774	−.0107	0.7147	.0286	0.2079	−.0179
	108	0.0165	.0035	0.8958	−.0125	0.0877	.0090
	112	0.0019		0.9713		0.0268	
	116	0.0002		0.9935		0.0063	
	120	0.0002		0.9987		0.0011	
	80	0.9962		0.0005		0.0033	
	84	0.9778	.0022	0.0013	.0054	0.0209	−.0076
	88	0.9049	−.0082	0.0116	−.0049	0.0935	.0032
	92	0.7280	.0120	0.0637	.0030	0.2083	−.0150
VII	96	0.4694	−.0127	0.2024	−.0324	0.3282	.0451
	100	0.2343	.0324	0.4377	−.0010	0.3280	−.0313
	104	0.0920	−.0320	0.6781	.0786	0.2299	−.0466
	108	0.0295	.0105	0.8481	−.0281	0.1224	.0176
	112	0.0078		0.9384		0.0538	
	116	0.0020		0.9769		0.0211	
	120	0.0003		0.9909		0.0088	

REACTION-TIMES TO AFFECTS ACCOMPANYING SMELL STIMULI

By F. L. Wells, Boston Psychopathic Hospital

From the point of view of the student interested in the time relations of sensory processes, the olfactory functions are a much less inviting field than the auditory, visual, or tactile. This aspect of them has apparently been neglected since the early days of experimental psychology. Efforts were made during this period, however, to develop a technique for the simple reaction to a smell stimulus. One method is to direct a blast of odoriferous air to the olfactory organs, with apparatus so arranged that the blast will simultaneously operate an electric circuit (*e.g.* Moldenhauer).[1] Another method, described by Zwaardemaker, is to actuate a tambour by means of the inspiration itself, directly, not through a pneumograph.[2] It does not appear that Zwaardemaker employed a chronoscope, though his apparatus is adaptable thereto, by such means as described below.

Brief comparison may be made of these two types of set-up from the present standpoint. An apparent advantage of the Moldenhauer technique is that the incidence of the stimulus is more accurately fixed. A disadvantage is that the end of the tube carrying the odor is introduced into the nose, a factor perhaps not serious for the simple reaction with which Moldenhauer was working, but very objectionable from the point of view of affective choice, which is here concerned. Nor would his technique allow a convenient alternation of the odors used as stimuli, as is necessary for the present experiments. This is also the chief objection to Zwaardemaker's set-up from the present point of view. Its advantage over Moldenhauer's is that the nose is left free for natural respiration, a factor to which Zwaardemaker attached much importance.[3]

From the point of view of simple reactions, the critic of Moldenhauer's set-up will also object to the possible noise of operating the air blast, and the pressure sensation from the blast, as distinct from the smell. Moldenhauer discounts these objections, maintaining that his *Ss* could and did react independently of them, and the reaction-time figures he quotes are consistent with this view. Nor does Zwaardemaker's technique wholly meet the situation. In a simple reaction experiment, *S* should not be giving himself the immediate stimulus. These matters are not so disturbing in the case of choice reactions, but as some comparison of the two is necessary, it is well to appreciate the difficulty under which the student of the simple reaction works.

*Accepted for publication March 18, 1928.

[1] W. Moldenhauer, Ueber die einfache Reactionszeit einer Geruchempfindung, *Philos. Stud.*, 1, 1883, 607-614.

[2] H. Zwaardemaker, *Physiologie des Geruchs*, 1895, 198.

[3] Other methods are described by G. Buccola, *La Legge del Tempo nei Fenomeni del Pensiero*, 1883, 44 f.; by G. Sergi, *Psychol. physiol.*, 1888, 294 f.; and by N. Vaschide, *Trav. du Lab., Asyle de Villejuif*, 1902.

In 1925 the writer published certain experiments with visual stimuli which support the finding that the time of distinguishing the pleasant or unpleasant quality of such stimuli is of the order of 0.8 sec.[4] At the same time (1922, 1923), experiments of like purport were made with olfactory stimuli. The two types of stimuli had compensating advantages and limitations. The visual stimuli are relatively easy to fix and time, but their affective qualities, in ordinary laboratory settings, are slight and of complex origin. The affective qualities of the smells available, on the other hand, were relatively strong and uncomplicated, but presented difficulties, to which allusion has been made, from the point of view of reaction-timing. For the point at issue, the utmost precision in timing was of less importance than the securing of relatively uncomplicated affects.

The odoriferous substances were placed in one-ounce stoppered bottles. Five were designed to give pleasant odors, and were perfumes purchased in the market. The trade names of the perfumes were "Lily of the Valley," "Cashmere Bouquet," "Mary Garden," "La France Rose," "Blasco." Five were supposed to give unpleasant odors, these being hydrogen sulphide (solution), putrid meat, oleic acid, capryl alcohol, butyric acid. In all experiments, these stimuli were exhibited in irregular and varied order. An electric fan was used to dissipate the odors after stimulation; this was a very necessary accessory.

Under normal conditions, the only point by which the incidence of a smell stimulus can be fixed, is the beginning of a phase of inspiration when such a source of stimulation is present. These experiments deal with the time elapsing between the beginning of a phase of inspiration and the designation of the feeling evoked as pleasant or unpleasant. This measurement was attempted in two ways. In a 'kymograph' series, a pneumograph was fitted to *S*, this actuating a tambour-pen upon smoked paper, with time-line. The bottle containing the substance to be smelled was placed beneath the nose during a period of expiration. When inspiration began the substance would be smelled, and the subject said as soon as possible whether the smell was 'good' or 'bad.' These words recorded a legible disturbance of the kymograph record. Measurement was made of the time elapsing between the record of beginning inspiration and the speaking of the response word. For general operating convenience, the speed of the drum was limited to about 10 mm. per sec. It was stopped between individual observations.

In the kymograph series 11 *S*s participated, in general the same individuals who took part in the visual experiments. The total number of these observations is 126, representing in general a single run of the series with every *S*. The general average time of reporting a stimulus as pleasant or unpleasant, approximates 0.88 sec. after the beginning of inspiration. The average time of every *S*'s reactions ranges between 0.7 and 1.38 sec.; the mean variation of these quantities is 0.22 sec. There was no determinable difference between the reaction-times for the pleasant and unpleasant odors, indeed the averages for the ten odors differ little, ranging from 0.83–0.96 sec., with mean variation of 0.05 sec.

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[4]F. L. Wells, Reactions to visual stimuli in affective settings, *J. Exper. Psychol.*, 8, 1925, 64-76.

The precision of sigma units is perhaps beyond the point at issue, but the attempt was made to measure the quantities concerned in this way. These experiments are the second or 'chronoscope' series. The critical element in this technique is a device to actuate an electric contact at the beginning of an inspiratory phase. Pneumographs have been devised to do this, but were not available at the time of these experiments. The plan adopted was to suspend in an oil bath, by a fine wire, a piece of sheet copper, approximately one inch square, gold-plated. Against the face of the copper sheet rested an aluminum writing lever, actuated in the horizontal plane by a tambour connected with *S*'s pneumograph. During expiration the metal pen pressed against the suspended sheet copper, moving it through the oil bath. Upon inspiration, the pen was drawn away from the sheet copper, which was inhibited from following it by the resistance of the oil. When the aluminum pen left the sheet copper, circuit was established through the chronoscope. This was a D'Arsonval galvanometer, set up according to Klopsteg. Reaction was with the right hand if the stimulus was felt as pleasant, with the left hand if unpleasant. The movement was to raise the hand from a telegraph key, breaking the galvanometer circuit.

This set-up appeared to function satisfactorily when the inspiration was a 'sniff,' and the *Ss* were instructed to make it so, but the effect of any lag not eliminated would be to shorten the reaction-time as read. Again, the 'sniff,' being sharper in this case than in the kymograph series, would induce the stimulus more promptly after stimulation began. It is perhaps due to a combination of these factors that the readings by this method are noticeably shorter than those in the kymograph series. The general average of the choice times in these observations is 555 sigma. The individual averages range between 369 and 905 sigma. The average of their mean variations is 109 sigma, relatively less than the variation in the kymograph series. Nine of the *Ss* participated. The total of observations was 161. As before, no difference is disclosed between the reaction-times of the pleasant and unpleasant stimuli.

In this series were observed a few 'contrary' reactions, as where a stimulus supposed to be pleasant was reported as unpleasant. In a total of twenty observations, one *S* found capryl alcohol pleasant both times it was given, and once found "La France Rose" unpleasant. He also had three false reactions, twice to hydrogen sulphide and once to oleic acid. By the remaining *Ss* one contrary reaction was recorded to hydrogen sulphide, oleic acid, and "Lily of the Valley."

Simultaneously with these experiments, series were run in which *S* was given now a bottle containing one of the odoriferous substances, now an empty bottle. The task was to react with the right hand if any smell, good or bad, was perceived, with the left if none was perceived. Eight of the *Ss* took part in these experiments, a total of 159 observations. There was the curious result that to react to a smell as distinguished from no smell commonly took longer than to react to it as pleasant or unpleasant. These times range between 387 and 965, with average at 615 sigma. The m.v. of the individual averages is 145 sigma. The average reaction-time to the empty bottle is 540 sigma, the m.v. of the individual averages 109 sigma. But in 3 of the 8 *Ss* the empty-bottle time was

longer than the smell time, so that little weight can be attached to the difference of their averages. Two of the 8 *Ss* concerned had shorter choice times to 'smell or none' than to 'pleasant or unpleasant.'

In the course of this series, there were three false reactions to the empty bottle, and one each to hydrogen sulphide and putrid meat. One *S* reported "after-images" of the smell sensations, which caused him to report smell in five of the ten cases where the empty bottle was presented to him. These were about one sixth longer than his other reactions, averaging 656 sigma, with m.v. of 93.

Techniques like Zwaardemaker's and the present, which depend for the stimulus on drawing the breath, are subject to the error of the time taken for the odor to reach the end-organ, after inspiration begins. Zwaardemaker discusses this point at some length.[5] This period of latency will vary a good deal according to the rapidity with which the breath is drawn. In his experiments it was apparently a sharp sniff, as in the present experiments of the chronoscope series. In the kymograph experiments the sniff was less pronounced, more *flairer* as he uses the term.

This may largely account for the shorter times observed in the chronoscope experiments; the substance being breathed in more sharply, reached the end-organ more quickly, and also in greater amount, causing more intense as well as more prompt stimulation. If the average of these readings, 555 sigma, be taken as representative, it bears a consistent relation to Moldenhauer's simple reaction readings, whose general average is 207 sigma. It is less consistent with Zwaardemaker's own readings (1895), which run 600–1000 sigma, or with those quoted by him from Beaunis, which average 546 sigma. The latter would be fairly consistent with the choice times recorded in the present kymograph series. But again, the shorter times quoted by Moldenhauer agree better with what is known of the simple reaction-time in general. These are some of the threads which must be disentangled before precise evaluation of data in this sphere is possible.

With reference to the main point of the inquiry,[6] however, the results seem sufficiently clear. They show correspondence in this field with the visual results, an affective choice time of less than one second, sometimes considerably less. In so far as affect may be looked on as a basic factor in emotion, it is obvious that such findings are opposed to any theory of emotions that refers them to processes having latent times longer than these. This, so far as now made out, is true of processes on which theories of the James-Lange type principally depend.

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[5]H. Zwaardemaker, *L'odorat*, 1925, 157-164.

[6]Cf. also E. A. McC. Gamble, Taste and smell, *Psychol. Bull.*, 13, 1916, 134-137; H. Henning, Psychologische Studien am Geruchsinn. *Handbuch der biologischen Arbeitsmethoden*, Abt. VI, Teil A, Heft 5, 1926, 797-802; J. H. Kenneth, Mental reactions to smell stimuli, *Psychol. Rev.*, 30, 1923, 77-79. E. Skramlik, Die Physiologie des Geruchs- und Geschmacksinnes, *Handbuch der Physiologie der niederen Sinne*, 1926, 336-339.

CONFIGURATIONAL PERCEPTION OF TACTUAL STIMULI

By B. L. Rosenbloom, Columbia University

The object of the following preliminary investigation[1] is to discover whether or not some of the principles found by the *Gestalt* school to govern visual perception are equally applicable to tactual perception. We propose to answer the following questions:

(1) Are some of the figures more easily perceived than others?
(2) If so, what are the characteristics of these stronger figures?
(3) Does an incomplete figure tend to be completed?
(4) Are the parts influenced by the whole figure?
(5) Does a figure have properties not contained in its parts?

Procedure. After applying the stimuli to a few skin localities, the palm of the hand was found best suited for the tactual stimulation of this experiment.

The figures used in this experiment were: a circle, which is designated in this article as (1); a square as (2); an equilateral triangle as (3); an incomplete circle as (4); three sides of a square as (5); a right angle as (6); and an incomplete triangle as (7), *i.e.* a triangle with one point missing. The area of each complete figure was 3.14 sq. in. The incomplete figures if completed also had the same area. The open circle was three-fourths complete, *i.e.* one-fourth of the circumference was missing. The two sides of the right angle were of the same length as the sides of the equilateral triangle. Of the open triangle the apex was missing. The circle, the square, and the triangle were selected because of their simplicity. The other figures employed in this experiment may be thought of as the circle, the square, and the triangle incomplete or open.

Every *S* was stimulated 5 times with each of the figures, a total of 35 stimulations. The stimulus was always applied to the same area of the palm but not always in the same position. For example, the open triangle was first presented with the open side facing the fingers, the second time the open side faced the side of the palm, the third time the open side faced the wrist, and so on. This order was maintained throughout the experiment for all *S*s. A time interval of at least 30 sec. elapsed between one stimulus and the following. Every *S* was stimulated 35 times at a single session, which lasted approximately one hour. Ten college students, 5 men and 5 women, served as the *S*s.

The figures were constructed by nailing brads through a thin wooden block four inches square. The brads were nailed closely together to form the figure desired. The *S* was stimulated by the dull ends of the brads. In order to avoid difference of pressure a one pound weight was placed on the top of each figure.

*Accepted for publication May 5, 1928.

[1]This is a summary of a paper submitted in partial fulfillment for a Master's degree at Columbia University.

By a preliminary experiment this weight was found to be sufficient, that is to say, it was found that greater pressure does not enhance perception and that a lighter pressure is not sufficient.

During the experiment the blindfolded *S* was seated comfortably on a chair with his arms in a cast-like holder, so that he was prevented from moving his arms. The holder also supported the back of the palm when the stimulus was applied. This was done to insure constancy of stimulation and to avoid possible kinaesthesis.

The *S*s were to describe or to draw the forms perceived, and were directed to make some response even if it be a guess. They were also to indicate the degree of certainty of their responses on a scale from zero to three, *i.e.* zero representing a mere guess and three absolute certainty.

There are three criteria by which one may judge the strength of a figure, namely, the number of times a figure is reported correctly (recognized), the degree of certainty of the correct responses, and the reaction-time of the correct responses. These different standards may also be used as a check upon one another. If one figure ranks high in the number reported correct but ranks low in reaction-time, one would be unable to come to a definite conclusion as far as the relative strength of the particular figure is concerned. On the other hand, if results of one standard corroborate those of the other standards the findings are more conclusive. These criteria may therefore be employed to discover the figure value of each of the seven figures. Each criterion was used as a check upon the other two.

TABLE I

SUMMARY OF RESULTS

1	2		3		4		5
Stimulus	No. correct reports		Reaction-time		Degree of certainty		Rank based on av. ranking
	Average	Rank	Average	Rank	Average	Rank	
Circle	80	1.5	4.1	3	2.8	1.5	2.
Square	50	5.	4.9	4	2.4	5.5	3.5
Triangle	80	1.5	4.	2	2.8	1.5	1.
Op. Circle	54	4.	5.4	6	2.3	9.	6.
Op. Square	40	6.	5.3	5	2.5	3.5	3.5
Rt. Angle	74	3.	5.6	7	2.4	5.5	5.
Op. Triangle*	6	7.	3.5	1	2.5	3.5	*

*Since the degree of certainty and the reaction-time for figure 7 were based on only three judgments, it should be ignored in this summary.

Results. Table I is a brief summary of the results. Column 1 contains the figures presented as stimuli; column 2 indicates the averages of correct reports and the ranking of each figure according to this criteria; column 3 shows the average and rank based on the reaction-time of correct responses; column 4 contains the average and ranking as indicated by the degree of certainty of the correct responses; and column 5 shows the rank based on the average ranking of each figure.

From this summary it is evident that the triangle and the circle are by far the best of the seven figures. In columns 2 and 4 they are of equal ranking. Only in column 3, reaction-time, the triangle gained over the circle by one-

tenth of a second. The square is the weakest of the closed figures. The three criteria give conflicting evidence regarding the figure qualities of the square; but as compared with the open figures it may be considered superior. The square and the open square are equal in figure qualities and rank next to the circle and the triangle; but if the degree of certainty were excluded in the average ranking since the average of one differs from that of the other by only one-tenth, the square would rank third undisputed. The triangle, the circle, and the square are all closed figures and it therefore appears that the complete figures are the stronger figures.

The unusual low average of the incomplete triangle in column 2 is partly due to the tendency to perceive the figure closed, *i.e.* as a triangle. The other criteria cannot be applied to this figure since they are based on only three out of the possible fifty judgments.

The open square is the most easily recognized of the open figures. The right angle ranks second among the open figures and the open circle third. If one is to judge by the number reported correctly, the only dependable criterion there is for the open triangle, that figure ranks lowest. On the whole it appears that the open figures are weaker and rank lower in figure qualities as compared with the closed figures. For a more detailed and more accurate ranking of each figure, the experimenter plans to make a more exhaustive study.

TABLE II
SUMMARY OF REPORTS

Stimuli	Response						
	Circle	Square	Triangle	Open Circle	Open Square	Right Angle	Open Triangle
Circle	40	0	0	4	0	0	0
Square	0	25	3	0	3	0	0
Triangle	0	0	40	0	0	0	1
Op. Circle	9	1	0	27	1	0	0
Op. Square	0	8	4	4	20	1	1
Rt. Angle	0	0	6	2	0	37	0
Op. Triangle	0	0	32	1	0	0	3

The next step in this experiment is to examine the records with the view to ascertain whether or not there is a tendency to perceive an open figure as closed. One need not look far to be convinced that this is the case. A large percentage of the wrong responses to all the incomplete figures were due to this tendency. Table II illustrates this tendency.

To discover whether or not a figure has properties of its own and if in turn it influences its component parts, it was decided to compare the two-point tactual threshold with the two-point tactual threshold when these points are part of a figure. If the threshold is different for the two points of an esthesiometer than for the two points when they are part of a figure, one is justified in concluding that the figure has properties of its own and that in turn it influences its component parts.

The first task was to measure the two-point threshold. The usual procedure by method of limits was followed. There were twenty ascending and twenty descending series. A time interval of about 30 sec. elapsed between successive stimulations. The same procedure was employed in obtaining the two-point

threshold when the points were part of a figure. *S* was first stimulated with the movable side of the triangle wide open, giving it the appearance of three sides of a square. The open side was gradually moved closer by degrees until the subject reported the perception of a closed triangle. *E* then stimulated him with the triangle completely closed, *i.e.* with the two points of the apex close together, and gradually increased the distance between the two points until *S* reported a perception of two points of an open figure. Twenty ascending and decending series were again used. To determine the two-point threshold a compass-esthesiometer with the dull end of brads for its points were used. The location chosen for this experiment was the part of the palm nearest the figures. A triangle was constructed in the same manner as in the first part of the experiment. This triangle had one movable side so that one could adjust it to be an open triangle as figure 7 of the first part of the experiment. The movable side was so arranged that it could be opened by degrees. The brads used in the construction of this triangle were the same as those used for the points of the compass.

In this phase of the experiment the results were more conclusive than in the previous one. All *S*s reacted similarly, though in different degrees. The two-point threshold, when the points are part of a figure, is greater than the two-point threshold when the points are not part of a figure. The average two-point threshold for the compass-esthesiometer was 8.4 mm., while the average two-point threshold for the figure was 19.4 mm. This phenomenon may be explained by the tendency to perceive the open figure closed; but, whatever the explanation may be, this much seems certain: the figure has properties of its own and influences its component parts.

Conclusions

(1) The closed figures are more easily perceived than the open figures. The square seems to be the weakest of the closed figures.

(2) The circle and the triangle have the best figure qualities of the seven figures stimulated on this area.

(3) The open figures tend to be perceived as closed. In this respect the open triangle showed the strongest tendency.

(4) The figure has properties of its own and influences its component parts.

IMPROVEMENT IN MEMORY SPAN

By Pauline R. Martin and Samuel W. Fernberger,
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Memory span has usually been regarded as a congenital ability. For this reason it has been considered highly valuable as a diagnostic test of mental ability. Humpstone has shown that memory span improves rapidly and markedly with increasing age up to the college level.[1] The implication is that the limits for adult memory span are fixed at birth and beyond this fixed limit, improvement is not to be expected. In Humpstone's own words: "We may assume that there is present a definite congenital capacity. This can never be increased." But then he goes on to say that "one's capacity can be used more and more so that its effectiveness is increased."[2]

It seemed worth while to ascertain the validity of these statements. For this purpose, two highly intelligent undergraduates at the University of Pennsylvania—Mr. W. F. Kennedy (K) and Mr. R. S. Rubinow (R)—volunteered to go through a lengthy and systematic series of memory span training.

The auditory-vocal memory span for digits was used under standard conditions. The digits were read at a rate slightly faster than one per second. Every day the *Ss* were given a complete series—from five digits until they broke down. Two series were given for each number of digits and the 'score' for each day was the highest single series reproduced absolutely correctly. New series were compiled and given each day.

K had 52 learning periods extending over a period of 4 months and R had 57 learning periods extending over the same period. Daily introspective reports were obtained every day from both *Ss*, especially regarding the methods employed during stimulation and during recall.

The daily scores will be found in Table I for K and in Table II for R. The scores for each 10 successive days will be found in a row. The right hand columns contain the averages for these 10 trials. The improvement in the case of K is very marked—the average for the first 10 series being 8.9 and for the fifth series of 10 being 13.1—an improvement of 47%. For R the improvement was not so great; his increase being from 10.9 for the first 10 series to 14.8 for the last 10 series—an improvement of 36%. It will be noted that R started with a higher memory span than K.

These values have been plotted as curves in the accompanying Fig. Both are of the form of typical learning curves. On the other hand, the curves for

* Accepted for publication July, 12, 1928.

[1] H. J. Humpstone, Some aspects of the memory span: A study in associability, *Univ. of Penna. Studies in Psych. and Ped.*, 8, 1917, 1-31.

[2] *Op. cit.*, 14.

the *Ss* show certain marked differences in form. The curve for K begins to improve markedly after the fourteenth daily series. That for R does not improve markedly until after the twenty-fifth series.

References to the averages in the right hand columns of Tables I and II will make this difference obvious. The average improvement for K is continuous. The improvement for R is slight for the first 30 series and very rapid for the fifth series of 10-day learning periods. Both *Ss* show a decrease in score at the end of the learning.

TABLE I

SHOWING DAILY SCORES FOR K

Days	Daily Scores										Ave.
1st 10	10	9	8	9	9	9	9	8	9	9	8.9
2nd 10	8	9	9	9	12	9	9	10	9	11	9.5
3d 10	10	10	11	10	11	13	12	14	12	13	11.6
4th 10	13	13	11	11	14	11	11	12	12	14	12.2
5th 10	13	13	13	11	15	11	16	13	15	11	13.1
Last 2	12	14									

TABLE II

SHOWING DAILY SCORES FOR R

Days	Daily Scores										Ave.
1st 10	12	10	9	10	11	12	12	10	11	12	10.9
2nd 10	12	10	11	10	12	12	12	11	12	11	11.3
3d 10	11	12	11	10	12	11	11	13	11	11	11.3
4th 10	11	12	13	10	13	12	11	13	11	12	11.8
5th	13	12	14	16	16	14	14	12	12	15	14.8
Last 7	15	15	13	12	13	14	14				13.7

How are these results to be interpreted? Witmer early identified memory span with the range of attention experiment when he said that memory span referred to the "number of discrete perceptions associated in a single act of attention, and the combination of the associated component parts of a single perception."[3] Since 1902, there has been a much more complete analysis of the old 'range of attention' experiment and of memory span. Both turn out to be at a higher level of mental process and to be much more complex than had been formerly supposed. The work of Oberly,[4] and Gill and Dallenbach[5] has shown that 'range of attention' is a misnomer for this experiment which, it turns out, may involve various levels of cognition and thus give various liminal values. Oberly, confirmed by Gill and Dallenbach, find normally three such levels: (1) an immediate reproduction; (2) grouping; and (3) re-imaging and counting.

Brotemarkle,[6] and Easby-Grave[7] have both shown that the mental processes underlying memory spans may be very different. They find an immediate

[3]L. Witmer, *Analytical Psychology*, 1902, 251.

[4]H. S. Oberly, The range of visual attention, cognition and apprehension, this JOURNAL, 35, 1924, 332-352.

[5]N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 256.

[6]R. A. Brotemarkle, Some memory span problems, *Psychol. Clin.*, 15, 1924, 251.

[7]C. Easby-Grave, Tests and norms at the six year old performance level, *Psychol. Clin.*, 15, 1924, 279.

reproductive process underlying the shorter series, for adults, while for the longer series there is grouping and organization. Easby-Grave questions whether the result of this later sort of process can be considered a true memory span.

More recently Oberly[8] has shown the relationship between the various levels of memory span and the same levels of 'attention' span.

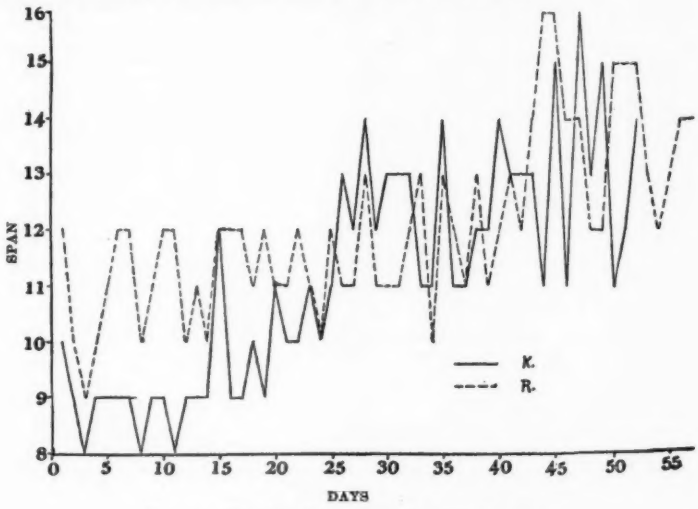

FIG. 1. Showing daily scores in memory span

Our introspective data show that the improvement all occurs at this higher level of organization. From the beginning of the training, R attempted to try various methods of grouping much more actively than K. This seems to account for R's more rapid improvement. It was only later in the experiment that K attempted this and then the improvement was marked in his case. This higher organization consisted primarily in attempting to increase the number of items in each group. This both *Ss* succeeded in doing and hence materially increased their span. Eventually both became proficient in grouping by 5s. The decline for both *Ss* at the end of the experiment was due, apparently, to an effort to increase the number of items in each group to six. In this they failed. Whether or not they might have succeeded had the experiment been longer continued cannot be predicted.

It is then rather obvious that no unqualified statement can be made with regard to improvement in memory span. If one means by memory span the number of items which may be immediately reproduced after a single, un-

[8]H. S. Oberly, A comparison of the spans of 'attention' and memory, this JOURNAL, 40, 1928, 295-302.

grouped act of attention, we can probably say very definitely that this is only slightly improvable with practice, if it is improvable at all. It may well be that we are here dealing with a congenital ability, the ultimate limits of which are fixed at birth.

On the other hand, the memory span obtained by grouping by the *S* during stimulation is highly improvable even in intelligent educated adults in whom a high degree of practice and competency in this process might be expected. This improvement is based on the development of more efficient methods of grouping and does not seem to depend on the utilization of new modalities of imagery or on any other factors which we have discovered.

It seems from our results that this grouping process is employed in all memory spans involving more than approximately 5 digits.[9] In the light of this improvement, which in one case is nearly 50% and in the other nearly 40% it would seem doubtful if we were here dealing with an ability whose limits were clearly congenitally fixed.

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[9]Cf. also Brotemarkle, Easby-Grave, *opp. citt.*

THE PROPERTIES OF SPACE IN KINAESTHETIC FIELDS OF FORCE

By C. O. Weber, Wells College, and K. M. Dallenbach, Cornell University

A wide difference seems to exist between the 'real' space which we traverse, and the Euclidean space studied in the classroom. The advent of relativism in physics marks the failure of the Euclidean geometry to harmonize with minute physical observations. The results of a recent study by Weber exhibit similarly, a corresponding failure of Euclidean geometry to fit psychological observation.[1] In the kinaesthetic sphere, at least, we must accept the truth of the most revolutionary of Einstein's conceptions, namely, that the actual properties of space and time depend on the presence and strengths of gravitational fields.

The results of Weber's study, in which *Ss* reproduced curvilinear distances and short time-intervals by movements of the forearm, lead to a tentative set of principles which seem to govern every individual's kinaesthetic experience of space. The most general finding was that the presence of forces against which the muscles must act, alters the phenomenal experience of spatial extent and temporal intervals. The effect of effort on space judgments was that a given distance traversed under a small load is phenomenally equivalent to a greater distance traversed under a lesser load. The effect of effort on time was, conversely, that a given interval of time under a given load seems phenomenally shorter than the same time under a lesser load. There was evidence also that, as load increases, critical points are reached where there are reversals of the errors caused by the small loads on both time and space judgments.

Since Weber's study was confined to curvilinear arm-movements and to short time-intervals his conclusions could not be generalized. It was in the hope, therefore, of obtaining a more complete 'geometry' of the 'muscle sense' that the present study was undertaken.

Preliminary Experiments

In the first series of experiments—which we designate because of their failure as preliminary—we attempted to discover by the method of average error the effect of load upon the kinaesthetic reproduction of visually presented triangles.

Triangles of 3 different sizes were presented visually, one at a time and in haphazard order. *S* attempted, while looking at the presented figure, to reproduce it by free arm movements with a stylus that was variously loaded.[2] He was not permitted at any time during the experiments to see his reproductions. After 100 drawings of every triangle under 3 different loads of the

*Accepted for publication November 15, 1927.

[1] C. O. Weber, Space and time in kinaesthetic fields of force, this Journal, 38, 1927, 597-606. The present study was conducted at the Cornell Psychological Laboratory during the summer of 1927.

[2] Our *Ss* were Dr. Theodora M. Abel (A), Mrs. C. O. Weber (W), and K. M. Dallenbach (D). W was relatively untrained in laboratory work.

stylus had been obtained, the angles and areas of the reproductions were measured respectively with a protractor and with a planimeter. The results show no constant effect of load. If load had any effect on the reproductions, it was obscured by variables over which we had no control.

The failure of the preliminary experiments may be due to the extreme variability of the factors involved in 'cross-modal' reference.[3] Though the visual standards were constant, we have no guarantee that the *Ss* had, from experiment to experiment, a constant kinaesthetic standard of reference. Indeed, it appears from our results that their kinaesthetic standard was constantly changing.

In order to avoid the complications of 'cross-modal' reference, we confined our work in the subsequent experiments to a single modality. The *Ss* made a movement, and then under varying conditions of load they made a second movement, and then attempted to judge it in terms of the first.[4] Both the standard and the variable fell, therefore, within the same universe of experience.

Main Experiments

Under the conditions laid down by the preliminary experiments we studied, and here report, the effect of load on judgments (1) of area, (2) of angle, and (3) of arc.

(1) *Area.* We employed the method of constant stimulus-differences, and used stimulus-areas of 5 different sizes, the middle one of which served in every case as the standard.

The stimulus-areas were square and varied in size according to the motor acuity of the different *Ss*.[5] Sizes were selected that gave, in preliminary trials with an unloaded stylus, nearly 100% correct reports for the extreme comparisons. The stimulus-areas varied for our most discriminating *S*, by steps of 10, from 20 sq. cm. to 50 sq. cm.; and for our least discriminating *S*, by steps of 15, from 20 sq. cm. to 80 sq. cm.

The areas, the perimeters of which were traced blindfold by the *Ss*, were demarcated by metal forms. Two right-angled metal flanges were attached to a metal base. One was fixed firmly to the base; the other was movable, and permitted rapid adjustment to any desired position. *S* used in tracing a square stylus which weighed 100 grm. The stylus was hollow, and its weight could be increased to 300 to 500 grm. by lead weights which *E* could quickly and noiselessly slip into place.

The form board was placed squarely in front of *S*. The stylus was placed within the enclosed area at the lower left hand corner. At a signal from *E*, *S* moved the stylus out along the left boundary, then across the top to the right, then down to the base, and then across to the starting point. The rate of move-

[3]Other experiments involving a 'cross-modal' reference have been conducted in the Cornell Laboratory. Attempts, for example, have been made to obtain an intensity of pressure equivalent to a standard sound, and contrariwise to obtain an intensity of sound equivalent to a standard pressure. The results, as yet unpublished, have shown difficulties similar to those encountered here.

[4]This method was suggested by the procedure used by H. L. Hollingworth. Cf. The inaccuracy of movement, *Arch. Psychol.*, 3, 1909, No. 13, 1-37.

[5]The *Ss* of the preliminary experiments served also in this series.

ment was left to *S*'s discretion. When the starting point had been reached, *E* changed the size of the area, or the weight of the stylus, or both, and *S* traced again, moving in the same direction, the perimeter of the stimulus-area. As soon as he had completed the movement he reported whether the area of the second square was greater than, equal to, or less than that of the first. Doubtful judgments were permitted, but the experiments giving doubtfuls were immediately repeated.

The standard and variable areas were compared by all the *S*s under the following conditions. (1) The standard area was given first and a stylus of 100 grm.—*i.e.* unweighted — was used to trace both the standard and variable areas. (2) A variable area was given first and a stylus of 100 grm. was used to trace both it and the standard. (3) The standard area was traced with a stylus of 100 grm., and then a variable area was traced with a stylus of 300 grm.—*i.e.*, weighted by 200 grm. (4) A variable area was traced with a stylus of 100 grm. and then the standard area was traced with a stylus of 300 grm. (5) The standard area was traced with a stylus of 100 grm. and then a variable area was traced with a stylus of 500 grm.—*i.e.* weighted by 400 grm. (6) A variable area was traced with a stylus of 100 grm. and then the standard area was traced with a stylus of 500 grm.

During the course of a single experimental period, the standard area was compared, under every one of the 6 experimental conditions, 5 times with every one of the variables. Thus giving a daily total of 150 judgments for every *S*. The comparisons were made according to a systematic arrangement[6] which insured an equality of practice for every one of the 6 experimental conditions.

The procedure was repeated on 5 successive days. The *S*s served at the same hour every day. We thus had a grand total of 750 judgments for every *S* or 25 judgments for every *S* under every one of the 6 experimental conditions for every one of the 5 variables. The number of judgments was sufficiently large to enable us to calculate difference limens for every *S* for every one of the experimental conditions,[7] and to determine from a comparison of the limens the effect of the various loads upon the *S*s' spatial perceptions.

Results. We present the results of our study of areas in Table I, which shows the upper and lower DLs for every *S* for every one of the 6 experimental conditions.

We shall first state, in the light of the results of Weber's study with linear distances,[8] the expected effect of load on the DLs, and then we shall turn to our results to see whether the expectation was justified. If an area traversed under load appears larger than the same area traversed without load, as Weber's results indicate, then we should expect to find that our standard area, when traced with the weighted stylus, would appear, ia relation to the variable area

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[6]E. B. Titchener, *Experimental Psychology, Instructor's Quantitative Manual*, 1905, 274.

[7]E. G. Boring, The number of observations upon which a limen may be based, this JOURNAL, 27, 1916, 315-319. L. B. Hoisington, An example of the fractionation of data from the method of constant stimuli for the two-point limen, *ibid.*, 28, 1917, 588-596.

[8]Weber, *op. cit.*

traced with the unweighted stylus, phenomenally larger, and that the effect of this would be shown in the data by an upward shift of the DLs: the upper DL would be larger and the lower DL would be smaller. Similarly, when the load was transferred to the stylus used in tracing the variable areas, the variable areas would appear, in relation to the standard, phenomenally larger, and the effect would be shown by a downward shift of the DLs: the upper DL would be smaller, and the lower DL would be larger. In brief we should expect to find that the upper DLs varied directly and the lower DLs inversely in size with the amount of the load.

The expected results were, as Table I shows, obtained. The upper DLs show a marked tendency for all the *Ss* to become smaller as the load decreases, and the lower DLs show, conversely, a tendency though less marked to become larger. Our results corroborate Weber's findings.

Inversions from the expected result occur, however, in the data of all our *Ss*. The progression of A's lower DLs is not smooth and regular; three inversions occur in that column. It is only when the extremes are combined and compared with each other and with the mean that the relation which we have pointed out becomes evident. D's data show a regular progression until we come to the negative extreme (experimental condition 5), where the stylus load is −400 grm. At this point there is an inversion in both DLs; the upper DL becomes markedly larger whereas it should have been smaller, and the lower DL becomes smaller whereas it should have been larger. W's data show inversions in both the upper and lower DLs when the stylus load is 0.

Weber also noted inversions among the results of his *Ss*. "Reversal of error," he observed, "occurs at different intensities of load for different *Ss*."[9] They are not, he believes, accidental, but mark critical points in the experiment. He offers, however, no explanation of them; and we also are unable to account for them. D's inversions at the lowest extreme of load may be due, as Garrison has suggested in his study on the perception of distance in terms of arm-movement,[10] to a shift in attitude and in the criteria of judgment which may be engendered when extreme values in the stimulus scale are presented. As the inversions in the results of the other *Ss* occur at very different points, this explanation would suffice only for D. Other explanations must, therefore, be sought. The inversions, however, do not occur in sufficient number, nor are they sufficiently large, to invalidate the general rule that the upper DL varies directly and the lower DL inversely with the load.

The effect of load is, to a slight extent, increased in our results by a time error. The temporal order of presenting the standard and variable stimulus-areas to the *Ss* was reversed in but one of the weight conditions, that with the unweighted stylus (experimental conditions 1 and 2). The results of these two series of experiments, which may be regarded as control series, show that the upper DLs are larger when the standard is given last. The difference is small for A and D, being respectively 0.451 and 4.845, but it is quite large (9.089) for W. The effect of the time error would be to increase slightly the

[9]Weber, *op. cit.*, 601.

[10]W. A. Garrison, The effect of varied instruction on the perception of distances in terms of arm-movement, this JOURNAL, 35, 1924, 420.

TABLE I

RESULTS OF STUDIES OF AREAS, SHOWING THE UPPER AND LOWER DLs IN SQ. CM., WITH THEIR MEASURE OF PRECISION (h), FOR EVERY *S*, FOR EVERY ONE OF THE SIX EXPERIMENTAL CONDITIONS

Time Order of Standard	Weight of Standard Stylus	Experimental Condition	Subjects											
			A				D				W			
			Upper DL	(h)	Lower DL	(h)	Upper DL	(h)	Upper DL	(h)	Upper DL	(h)	Lower DL	(h)
Last	+400	6	7.405	.115	1.343	.042	23.925	.066	3.437	.046	14.080	.076	1.389	.081
Last	+200	4	6.606	.088	0.163	.083	21.524	.039	7.648	.036	14.564	.085	1.134	.075
Last	0	2	5.279	.048	3.750	.056	13.993	.051	8.125	.102	10.569	.134	13.628	.012
First	0	1	4.828	.109	0.793	.106	9.048	.049	9.953	.054	1.480	.077	13.841	.065
First	−200	3	5.917	.111	1.366	.088	4.769	.088	10.383	.037	3.375	.129	7.161	.054
First	−400	5	4.209	.077	6.076	.055	10.202	.055	7.485	.044	1.709	.071	10.631	.190

size of the upper DLs when the standard is given last, and to decrease their size slightly when the standard is given first. The amount of the time error is not sufficiently large, however, for any of the *Ss* (if the size of the time error for the other weight series may be estimated from that of the control series)[11] to alter materially our results. Our results stand, when corrected for the time error, just as unequivocal as before.

(2) *Angle.* The procedure was essentially the same as that employed with area. We used 5 metal isosceles triangles, with apical angles of 40°, 50°, 60°, 70°, and 80°, to demarcate the movements of the *Ss*. The 60° angle served as the standard, and the method of constant stimulus-differences was again employed.

The triangular form used in a particular case was placed directly in front of *S* with the apical angle away from him. A cylindrical metal stylus was placed by *E* within the metal form at the left base-angle. At a given signal, *S*, who was blindfolded, grasped the stylus, moved out along the left side of the triangle to the apex and then down the right side to the base, that is he traced the angle along the inner side.[12] *E* then substituted another form, or another stylus, or another form and stylus, and the procedure was repeated. After *S* had traced the second angle, he reported whether it was greater than, equal to, or smaller than the first. Experiments that gave doubtful judgments were immediately repeated.

Styluses of 3 different weights, 100, 200, and 300 grm., were used by the *Ss* in tracing the angles. In every other respect except weight the styluses were alike.

Since the stylus penetrated the larger angles further than it did the smaller, the sides of the various pattern triangles were constructed of different lengths. The lengths were carefully adjusted so that the linear distances traversed by the *Ss* in tracing the various patterns were exactly the same. Outward and return movements were the only movements made by the *Ss*; they were never permitted to move the stylus across the base.

With the exception of the differences in load of the styluses the variable conditions described in the previous section were repeated here.[13] The *Ss*[14] served on 5 successive days, and at the same hour every day. Twenty-five comparisons were made of the standard and every variable by every *S* under every one of the 6 experimental conditions.

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[11]It would, doubtless, have been better had we taken account of this error in all of our weight conditions, but time was short and to have introduced four more series of experiments into our procedure would have meant the curtailment of our summer's program.

[12]We at first tried tracing the angles on the outer side, but we soon abandoned that method, because the *Ss* were unable to follow the angular pattern. When they reached the apex of the stimulus-form, their stylus slipped away from the limiting guide, and the 'free' movements involved in the search and return to the guide destroyed the kinaesthetic pattern which was the basis of their judgments.

[13]*Supra*, 97.

[14]We used in this experiment, in addition to D and W, who served in the first experiment, two other *Ss*: Dr. Junichiro Horiguchi (H), and Mr. Han Kao (K), both trained in laboratory work.

Results. We present the results in Table II, which shows the upper and lower DLs and their measures of precision (h) for every *S* for every experimental condition. Before turning, however, to the discussion of our data we shall state the expected result. If it takes more effort to make a turn at an acute than an obtuse angle, then an angular turn under load should appear more acute, since more effort would be required to negotiate it, than the same angle without load. If this reasoning is correct the upper DLs in Table II should vary inversely with the load, and the lower DLs should vary directly with the load. The explanation is as follows: when the standard angle is traced with a stylus heavier than that used in tracing the variable angles with which the standard is compared, the standard angle appears more acute, *i.e.* phenomenally smaller, and the effect is shown in a downward shift of the DLs; the upper DL becomes smaller, and the lower becomes larger.

A clear confirmation of the expected result was not obtained. Indeed, a cursory inspection of Table II would lead one to conclude that there was no correlation between prediction and result. A closer examination reveals, however, some points of agreement. In the first place, the prediction regarding the upper DLs holds for all the *Ss* in the series in which the standard was given first (experimental conditions 1, 3, and 5). The upper DLs in these series increase in size as the load decreases. Furthermore, the prediction regarding the lower DL holds, in the same series of experiments, for two of our *Ss*, D and W. Their lower DLs become smaller as the load decreases. There are also some points of agreement between prediction and result, in the series in which the standard was given last (experimental conditions 2, 4, and 6), The upper DLs of D, K, and W are smaller with the +200 grm. load than with the +100 grm. load; H's upper DL is smaller with the +100 grm. load than with 0 load; all of W's lower DLs follow prediction; and there are also instances in the lower DLs of the other *Ss* in which the prediction is realized.

Instances of agreement between prediction and result are numerous enough if the upper and lower halves of the table, that is the two time orders, be considered separately. Disagreement becomes pronounced only when the table is considered as a whole. This may be due in part at least to the time error which is large for D, H, and W. It is significant that of all the *Ss*, K's time error is the smallest and his results stand in the closest agreement to prediction.

(3) *Arc.* We used the method of constant stimulus-differences as before, and 5 metal arcs as stimulus-patterns to demarcate the arm-movements of our *Ss*. Arcs were constructed that gave equal steps in curvature. After a short preliminary trial with various curvatures we decided upon arcs whose radii were 6.0, 6.66, 7.5, 8.57, and 10.0 cm. Though unequal in terms of radius-length, the steps are equal in terms of curvature, since curvature varies with the reciprocal of the radii. The arc of 7.5 radius was used as the standard. The arcs were all cut to the length of 20 cm. The linear distance traversed in tracing the various arcs was, therefore, the same in every case.

The arcs were placed directly in front of *S* with the concave side toward him. *E* placed a cylindrical metal stylus of 100, 200, or 300 grm. weight at the left end of the arc. *S*, who was blindfolded, grasped the stylus at a given signal and moved it along the concave side of the arc, at a rate of movement suitable to him, until he reached the limiting block at the right end of the arc, at which

TABLE II

RESULTS OF STUDY OF ANGLES, SHOWING THE UPPER AND LOWER DLs IN DEGREES, WITH THEIR MEASURES OF PRECISION (h), FOR EVERY *S*, FOR EVERY ONE OF THE SIX EXPERIMENTAL CONDITIONS

Time Order of Standard	Weight of Standard Stylus	Experimental Conditions	Subjects															
			D				H				K				W			
			Upper DL	(h)	Lower DL	(h)	Upper DL	(h)	Lower DL	(h)	Upper DL	(h)	Lower DL	(h)	Upper DL	(h)	Lower DL	(h)
Last	+200	6	8.40	.077	6.48	.042	7.07	.141	2.11	.157	1.86	.052	8.61	.046	24.99	.066	2.04	.042
Last	+100	4	10.63	.057	7.68	.067	5.00	.067	0.94	.054	3.06	.052	10.03	.047	27.41	.034	2.71	.052
Last	0	2	7.18	.055	3.24	.038	17.03	.100	1.31	.054	0.44	.070	7.22	.059	15.01	.041	11.87	.048
First	0	1	3.27	.108	8.46	.102	4.18	.073	2.87	.071	0.53	.069	4.80	.075	3.69	.056	8.97	.111
First	−100	3	7.66	.123	7.07	.108	4.32	.113	2.56	.165	0.69	.068	5.42	.067	6.57	.118	8.48	.070
First	−200	5	8.37	.061	6.39	.058	15.95	.100	6.76	.074	2.53	.062	5.65	.060	7.94	.091	5.85	.070

TABLE III

RESULTS OF STUDY OF ARCS, SHOWING THE UPPER AND LOWER DLs IN TERMS OF *x*, WITH THEIR MEASURES OF PRECISION (h′), FOR EVERY *S*, FOR EVERY ONE OF THE SIX EXPERIMENTAL CONDITIONS

Time Order of Standard	Weight of Standard Stylus	Experimental Condition	Subjects															
			D				H				K				W			
			Upper DL	(h′)	Lower DL	(h′)	Upper DL	(h′)	Lower DL	(h′)	Upper DL	(h′)	Lower DL	(h′)	Upper DL	(h′)	Lower DL	(h′)
Last	+200	6	1.53	.258	1.55	.245	0.16	.426	0.31	.411	1.49	.081	1.04	.120	2.13	.170	0.23	.222
Last	+100	4	1.27	.403	1.49	.197	0.09	.372	0.52	.404	2.75	.094	0.02	.078	0.42	.106	0.16	.205
Last	0	2	1.32	.437	1.73	.141	0.25	.795	0.58	.354	1.23	.145	0.52	.159	1.94	.378	0.42	.106
First	0	1	0.93	.256	1.09	.390	0.35	.423	0.20	.531	0.18	.381	0.47	.338	0.56	.465	0.92	.807
First	−100	3	0.53	.290	1.09	.236	0.66	.484	0.34	.427	0.03	.344	0.36	.307	0.37	.454	0.74	.360
First	−200	5	0.59	.388	0.88	.256	0.56	.428	0.20	.503	0.35	.318	1.17	.293	0.26	.443	0.75	.446

point he released the stylus and placed his hand and arm in the ready position. The procedure was then repeated with another weight of stylus, or another degree of curvature, or with a change of both, and then *S* reported whether the curvature of the second arc traced was greater than, equal to, or less than that of the first. As in the previous experiments, doubtful judgments were permitted, but the experiments giving them were immediately repeated.

The experimental variations used in the experiments with areas and with angles were used again with the arcs. The same *Ss*, as in the experiment with angles, served on 5 successive days and at the same hour every day. Twenty-five judgments of the comparison of the standard and every one of the 5 variable arcs were received from every *S* under every one of the 6 experimental conditions.

Results. Upper and lower DLs and their measures of precision (h) were computed for every *S* for every experimental condition. These data are given, in Table III, in terms of x in place of D. The inverse relation between radius and curvature, and the lack of a simple relation between them, renders the transformation of the x values into D values problematical. Since we are concerned, not with the absolute size of the DL, but merely with their relative sizes, the x values serve our purpose as well as the D values.

The expected course of the DLs with arcs is the same as the DLs with areas; the upper should vary directly, and the lower inversely with the weight of the standard stylus. This prediction follows from our previous results. If an area traversed under load appears phenomenally larger, and an angle traced under load phenomenally smaller, *i.e.* more acute, then we should expect that the curvature of an arc traversed under load would appear phenomenally greater.

The expectations are realized, as Table III shows, in the results of three of our *Ss*, D, K, and W. The upper DLs of these *Ss* vary, with but few inversions, inversely with the load of the standard. The inversions, where they do occur, are small and appear for the most part at the extremes of the weight-series. D's data show one slight inversion, which occurs at the lowest weight extreme, −200. K's data show two inversions, both slight and both occurring at the weight extremes. W's data show one inversion, which is not large and also occurs near the upper weight extreme. The data for the lower DLs are less uniform, but the predicted result, direct variation with load, is evident in them, particularly when the extremes are compared with each other and with the mean.

H's results stand in absolute disagreement with expectation and with the results of the other *Ss*. His upper DLs vary inversely and his lower DLs tend to vary directly with load. It is only fair to state, however, that H had difficulty in understanding what was meant by greater curvature. We are inclined to believe that 'greater curvature' meant for him the curve of a greater circle. His 'greater' would correspond to the 'lesser' of the other *Ss*, and his 'lesser' with their 'greater.' If his results are interpretated in this light they confirm rather than negate, prediction, and corroborate rather than disagree with the results of the other *Ss*.

The apparatus used in all these experiments is not as delicate as the measurement of the illusion under investigation demands. Load is not able to exert its maximum effect when metal patterns are traced. In order to overcome the frictional resistance, *S* exerts an increased effort that is greatly in excess of

that demand, and consequently the demands made by the weight-conditions are partially met and, to the extent that they are met, obscured. Free movements would show the demands of load better than those restricted by forms, but the difficulty of using free movements immediately becomes apparent when an attempt is made to bring them under experimental control.

Conclusions

It is a common error to suppose that the new school of physical relativists have introduced subjectivism into physics. Curiously enough, the relativists themselves contend that they have saved the objectivity of physical phenomena.[15] There are two criteria of objectivity involved here. There is, first, the old absolutist notion that the objective is that which is independent of all observers. If this criterion is accepted, then surely Einstein is a subjectivist, for his central thesis is that the quantitative traits of space and time depend on the observers. But this criterion of objectivity is wholly useless, if not meaningless. It is difficult, even self-contradictory, to form an idea of what the universe, independent of all observers, would be like. At any rate, the question of the existence of such an independent order has been the threadbare enigma of philosophers since Protagoras, and is still the chief bone of contention between the new and the critical realists of today. There is another criterion of objectivity which leaves science free to winnow the 'real' from the 'unreal' without becoming enmeshed in metaphysical tangles. This true criterion of objectivity is that the objective is independent of any particular observer, not that it is independent of all observers. Einstein's great accomplishment was to prove that it was possible to find a common denominator between the time and space experiences of all observers. This common denominator is a law. The objective is that which is governed by law: if my experience of space rests on a general law, it is more than my private subjective experience.

The restless insurgents in psychology who espouse behaviorism often reject phenomenological data because they are 'subjective.'[16] Perhaps no two human beings exist who get the same quantitative experience in traversing a mile. Consequently the 'objective' psychologists would prefer to compel agreement by measuring the mile with a yard stick. This, of course, secures a certain amount of agreement, but not psychological agreement. To count the number of yards in a mile is only a way of avoiding a phenomenal experience. To add absurdity to absurdity, the geometer, whose task our behaviorist has usurped, should now decide that it was his business to study distances as phenomenal experiences! The 'mentalists' in psychology will take no part in this comedy because they do not hold the naïve notion that a thing, in order to be objective, must be 'really out there' independent of all observers. Whatever the variety of our kinaesthetic experience of space, that variety is still within the bounds of law. What these laws are, we have been trying to establish in this study.

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[15]Cf. A. S. Eddington, *Space, Time, and Gravitation*, 1921, Preface.

[16]Thus J. R. Kantor hails Weiss's *Theoretical Basis of Human Behavior*, as a triumph of 'objectivism' in psychology because Weiss regards psychological phenomena as ultimately 'electron-proton configurations.' Cf. *J. Phil.*, 24, 1927, 22.

From the results of these experiments the following generalizations may be drawn.

(1) A given area sensed under load appears to be phenomenally enlarged.

(2) A given angle traversed under load appears to be phenomenally more acute, *i.e.* smaller.

(3) A given arc traversed under load appears to be phenomenally more sharply curved, *i.e.* to be a curve of smaller radius.

(4) As load is increased, critical points are reached where the results due to load are reversed.

We acknowledge the tentative character of our results. They suggest, however, that the geometry of the muscles is hyperbolical, that it is the geometry of Lobachevski. We suggest that experimental study will show that visual space is also non-Euclidean. Unlike Euclid's geometry, parallel lines meet in visual space. In visual space the degree of angle and curvature of line vary with perspective. Unlike Euclid's geometry, visual space has one point which is always unique, that occupied by the observer.

INTELLIGENCE AND PIGMENTATION OF HAIR AND EYES IN ELEMENTARY SCHOOL CHILDREN

By G. H. Estabrooks, Colgate University

The following extracts deal with material which the writer collected for the Harvard Growth Study in the years 1924–1926. One of them shows a definite correlation between pigmentation and 'intelligence' which is seen to be spurious only when certain factors, very easily overlooked, are taken into account.

The pigmentation of hair and eye was secured for the entire elementary school population of New Rochelle, N. Y., in Grades VI, VII, and VIII. The work was done without the aid of a color scale and consequently is somewhat subjective; but we are concerned here only with the gross divisions of light hair-dark hair and of blue eye-brown eye, and thus we eliminate those finer distinctions wherein confusion would be most liable to arise. All the children had previously been given the Dearborn 'C' Group Test.

TABLE I

Median Intelligence Quotients
(Dearborn 'C' test)

Pigmentation Groups	Boys		Girls	
	No.	I.Q.	No.	I.Q.
Blue eye	275	108.9	276	109.2
Brown eye	116	105.0	126	105.5
Mixed eye*	53	——	41	——
Light hair	214	109.1	220	109.2
Dark hair	230	106.3	223	105.1
Total	444	107.7	443	107.1

*Mixed eyes are included merely to balance the numbers

In a previous article[1] the writer has pointed out why we cannot compare the intelligence quotients of different linguistic groups; but here we shall consider only the relation between pigmentation and intelligence within the North European group of these school children. Membership in this group requires both parents to be of North European extraction (stock originating in the British Islands, North Germany, or the Baltic States). Linguistic handicaps within such a school group in a city like New Rochelle are rare and probably insignificant.

Median intelligence quotients from the Dearborn 'C'-Test are given in Table I. The children are divided into gross pigmentation groups.

*Accepted for publication May 24, 1928.

[1]G. H. Estabrooks, A proposed technique for the investigation of racial differences in intelligence, *Amer. Natur.*, 62, 1928, 76-87.

The blond child is definitely in the lead as regards 'intelligence.' We must consider, however, the fact of progressive pigmentation. A sample such as this naturally contains a wide range of ages, actually in this case running from 9–18 years. Owing to selective processes going on within the elementary grades we know that the younger children will, as a whole, be the more intelligent. The bright children will have passed on and a number of older retarded children will swell the ranks of the group with low intelligence. We should also strongly suspect the presence of progressive pigmentation. In other words, these younger children would be not only more intelligent but also more blond.

Table II gives the percentage of each pigmentation-group for each successive year.

TABLE II

PERCENTAGE OF EACH PIGMENTATION-GROUP FOR EVERY AGE GROUP

HAIR

Age Group	Boys		Girls	
	Light	Dark	Light	Dark
Nine	0.5	0.0	0.56	1.8
Ten	3.8	2.6	10.1	7.6
Eleven	19.8	14.0	22.0	12.2
Twelve	33.0	27.3	29.3	27.0
Thirteen	24.5	22.4	22.5	29.8
Fourteen	12.2	18.0	10.5	12.6
Fifteen	5.4	11.8	4.6	7.2
Sixteen and over	0.8	3.9	0.5	1.8

EYES

Age Group	Boys		Girls	
	Blue	Brown	Blue	Brown
Nine	0.3		1.1	
Ten	3.3	0.8	10.8	5.0
Eleven	16.5	13.4	18.2	17.3
Twelve	29.7	36.2	29.8	23.2
Thirteen	26.0	17.6	23.9	31.3
Fourteen	14.0	16.0	10.8	13.2
Fifteen	8.4	10.1	4.0	9.2
Sixteen and over	1.8	5.9	1.4	0.8

The tabulated results show quite clearly a progressive pigmentation within this group and there can be little doubt that this stands at the basis of the positive results.

As a check, let us take another sample of North European children from the school systems of Medford and Revere, Massachusetts. Here we have an entire group at one age, the eighth year. They were given the Dearborn 'A' Group Test within a week of these observations.

Table III contains their median intelligence quotients. It indicates that no pigmentation-class had any superiority in intelligence as measured by this test. These results are typical of those obtained throughout the entire investigation at Medford and Revere. The Otis Group Test, the Primary Examina-

TABLE III

MEDIAN INTELLIGENCE QUOTIENTS OF CHECK GROUP
(Dearborn 'A' test)

Pigmentation Groups	Boys		Girls	
	No.	I.Q.	No.	I.Q.
Blue eye	85	104.9	132	105.8
Brown eye	33	112.6	37	111.2
Mixed eye	21	——	16	——
Light hair	78	104.7	112	106.6
Dark hair	61	104.8	73	108.2
Total	139	104.8	185	107.1

tion, Myer's Mental Measure, and the Stanford Revision likewise yielded negative results. In children of these ages, then, there is no demonstrable relationship between pigmentation and 'intelligence.'

PUBESCENCE AND PLAY BEHAVIOR

By Paul Hanly Furfey, Catholic University of America

The present writer[1] has already drawn attention to the relation existing between pubescence and the age at which boys lose their interest in Scouting. The parallelism of the two curves suggested that there was some causal connection between them. Lehman,[2] working independently, has shown that the same thing is true of girls and the age at which they lose their interest in doll play.

The accompanying diagrams, Figs. 1–6, were prepared in an effort to discover whether this fact would be equally true of some other play activities. They were based on a test given to about 450 grammar school and high school boys in Washington, D. C., in May 1926 during the standardization of a scale for developmental age elsewhere described.[3]

The boys taking the test were asked to read a long list of activities and to check those they liked. Then they were asked to read the list through again and to check a second time those which they liked very much. In computing the results the items checked twice were arbitrarily weighted twice as much as the other items.

TABLE I

Number of Boys Checking Certain Activities at Specified Ages, Expressed as Percentages of the Number Checking at the Age of 12 Years

Play Activity	Age Groups in Years				
	12	13	14	15	16
Cops and thieves	100	108	61	22	15
Run, sheep, run	100	113	74	38	6
Follow the leader	100	91	54	44	15
Tag	100	87	57	52	18
With marbles	100	91	59	36	19
With electric trains	100	77	53	23	6
Spin tops	100	72	37	23	14
Fly kites	100	83	43	27	14
With Meccanos	100	100	63	40	19
Walk on stilts	100	80	62	47	17
With bows and arrows	100	84	49	31	13

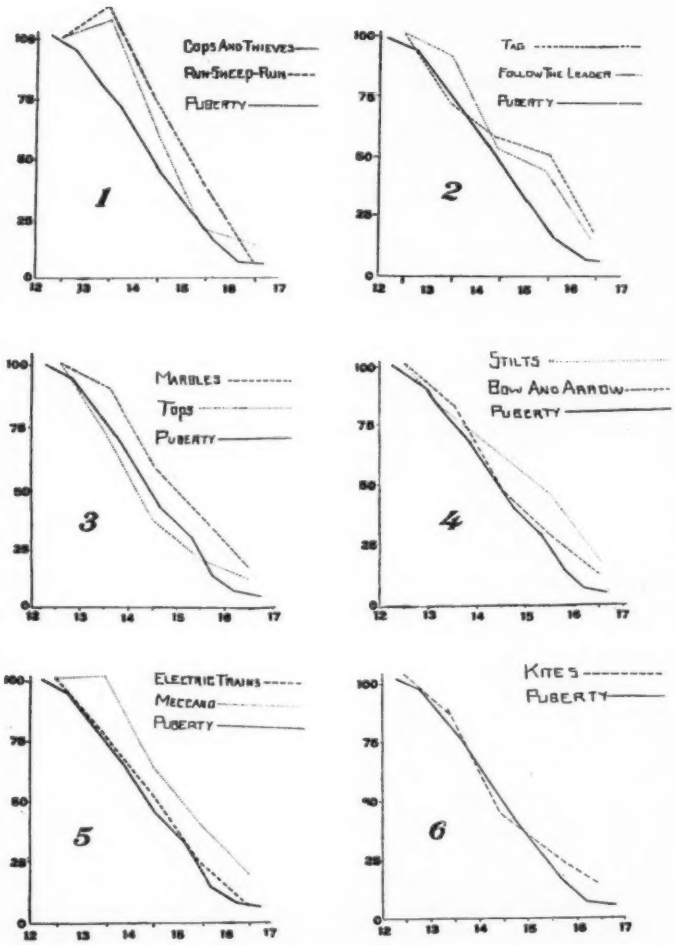
In presenting the results herewith they are expressed in percentages of the 12-year-old score. It was felt that this procedure would give a truer picture of the situation than if the results were expressed in percentages of the total

*Accepted for publication July 20, 1928.

[1] P. H. Furfey, *The Gang Age: A Study of the Preadolescent Boy and His Recreational Needs*, 1926, 10-13.

[2] H. C. Lehman, A study of doll play in relation to the onset of pubescence, *Ped. Sem.*, 34, 1927, 72-76.

[3] P. H. Furfey, The measurement of developmental age, *Cath. Univ. Amer. Educ. Research Bull.*, 2, 1927, no. 10.



FIGS. 1–6. NUMBER OF BOYS CHECKING CERTAIN PLAY ACTIVITIES AT THE DIFFERENT AGE GROUPS, EXPRESSED AS PERCENTAGES OF THE NUMBER CHECKING AT THE AGE OF 12 YEARS

The age groups are shown on the abscissae, the percentages on the ordinates. The solid line represents the proportion of boys who have become post-pubescent at the different ages

group. For the point under discussion is not what proportion of 12-year-old boys are interested in a given activity, say, playing tag, but rather the proportion of those boys who are interested in tag at the age of 12 who will give it up at pubescence. The proportions are presented in Table I.

In the accompanying diagrams the solid line represents the proportion of boys who have become post-pubescent at the specified ages. The data are taken from Crampton's study of 3835 New York City high school boys.[4] The criterion of post-pubescence was the appearance of a kink or twist in the hair of the pubis. This is probably the best feasible criterion of puberty.

The dotted lines represent various activities. These 11 activities—playing 'cops and thieves,' 'run, sheep, run,' 'follow the leader,' tag, and with marbles, electric trains, tops, kites, Meccanos, stilts, and bows and arrows—were selected because they all showed the common characteristic of declining sharply at the age of 12 years. It will be noted that they are predominantly activities of a dramatic, manipulative, or individualistic type.

It is unwise to generalize too freely on these data, but they certainly suggest that the sudden loss of interest in these activities is due to pubescence.

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[4]C. W. Crampton, Physiological age: a fundamental principle, *Amer. Phys. Educ. Rev.*, 13, 1908, 141-154, 214-227, 268-283, 345-350.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

LVII. Experiments on the Relation of Reaction-Time, Cube Fluctuations, and Mirror Drawing to Temperamental Differences

By M. F. Washburn, K. Keeler, K. B. New, and F. M. Parshall

In several recent studies from this laboratory groups of observers have been formed representing opposite extremes of certain temperamental traits, by the method of having each individual rated by herself and three of her friends independently on a scale representing four degrees of the trait in question, and given a score equal to the sum of these numerical ratings. In the present study we have tried to find whether to such groups there correspond (a) differences in speed of ordinary reactions to noise stimuli, (b) differences in variability of such reaction-times, (c) differences in the rapidity of the fluctuations of an outline cube figure, (d) differences in the number of errors made in the first mirror tracing of an outline star, and (e) differences in the speed of movement in this mirror tracing, represented by the total time required to trace the star, divided by the number of errors made.

Observers. All the *O*s were young women college students. The groups investigated were as follows:

(1) An emotional (E+) group of 33 *O*s and a calm (E−) group of 31 *O*s. These were formed by having each *O* rate herself, and requiring each of her three friends to rate her, 1 if she was very emotional, 2 if emotional rather than calm, 3 if calm rather than emotional, and 4 if very calm, in general temperament. The highest grade of emotionality to be reached by adding such ranks would be 4, if the *O* herself and all three of her friends considered her very emotional; the lowest possible grade would be 16. The E+ group consisted of *O*s with scores from 4 to 8 inclusive; the E− group of *O*s with scores from 12 to 15 inclusive (no one had a score of 16).

(2) An irritable (I+) group of 35 *O*s and a non-irritable (I−) group of 41 *O*s, obtained in precisely the same way on the basis of ratings for tendency to irritation.

(3) A timid group (F+) of 28 *O*s and a fearless group (F−) of 34 *O*s, obtained in the same way on the basis of ratings for tendency to fear.

(4) An extravert group of 51 *O*s and an introvert group of 55 *O*s, obtained by having each *O* answer for herself, and two of her friends answer for her, the questionary on extravert-introvert traits devised by L. R. Marston.[1] An

*Accepted for publication July 18, 1928

[1] L. R. Marston, The emotions of young children, *Univ. Iowa Studies in Child Welfare*, 3, 1925, no. 3.

O was ranked as an extravert if the total number of extravert answers was at least 1.5 times the total number of introvert answers; and as an introvert if this ratio was reversed.

Tests. The tests applied to these groups were as follows:

(1) Speed of first 10 reactions to noise. The Sanford vernier chronoscope was used and 20 reactions made by each *O* to the sound of metal rapping on the long pendulum key. The times for the first and second tens were averaged separately, but only the average for the first 10 reactions was considered in examining the results.

(2) Variability of first 10 reactions to noise. The average deviation of the first 10 reaction-times was taken, and divided by the average reaction-time to find the percentage deviation, in the case of each *O*.

(3) Fluctuations of an outline cube. A cube 2 cm. a side was drawn in ink on white cardboard and placed on the table in front of *O*, who was told to look steadily at it, to hold down a telegraph key while she perceived the cube as resting on the ground, and to release the key when she perceived the cube as hanging in the air. These movements of the key were recorded on a kymograph drum, along with a time line from a Jacquet marker. The average duration of the 'down' phases during two complete revolutions of the kymograph, requiring about 3½ min. (the speed of the kymograph was not quite constant), was found for each *O*.

(4) Mirror drawing: speed and errors. Under the standard conditions described by Whipple,[2] each *O* traced a star by mirror drawing, the time and number of errors being recorded. Account was taken of the speed of movement (roughly represented by time divided by errors), and of the number of errors.

These tests were chosen because it seemed *a priori* possible that temperamental differences in *O*s might be especially influential on their results. When the work was begun, we had not happened to notice McDougall's reference to experiments of his which indicated a very definite relation between speed in the fluctuation of ambiguous figures and extravert or introvert tendencies. He says, "The introvert subjects experienced rapid fluctuations, while the extraverts displayed a slow rate of alternation. The number of my subjects was far too small, but the indication was that the experiment reveals the position of the subject in the introvert scale."[3] It had seemed to the senior author of the present study that some relation might well exist between temperamental differences and *speed*, *variability*, and *accuracy* of performance. Reaction-time, cube fluctuation, and mirror drawing suggested themselves as simple processes involving these factors. It also appeared probable that temperamental differences would have more effect on the early performances in a series, before the process of mechanization had set in; therefore account was taken of only the first 10 reaction experiments and of only one mirror drawing.

Results. As might have been expected from the complexity of the phenomena with which we were dealing, the differences that appeared were, when stated in terms of averages and average deviations, not statistically reliable;

[2]G. M. Whipple, *Manual of Mental and Physical Tests*, II, 1915, 121 f.
[3]William McDougall, *Outline of Abnormal Psychology*, 1926, 444 f.

the individual variations were too large. Nevertheless certain tendencies appeared. The best way of demonstrating these seemed to be the following. For a certain test, the results from the entire group of 132 *O*s were divided into quartiles.[4] Then for each of the subgroups, for the emotional subgroup, say, the number of *O*s found in each quartile of the entire group was counted, and expressed in terms of percentage of that subgroup. For example, when all the 132 *O*s were arranged in quartiles on the basis of their average speed in the first 10 reactions, 24.2% of all the E+ *O*s were found to be in the first quartile, 30.3% in the second quartile, 30.3% in the third quartile, and 15.1% in the last or slowest quartile. A tendency on the part of *O*s belonging to this subgroup to react quickly rather than slowly would be indicated by a considerably larger percentage in the first than in the last quartile. 'Considerably larger' ought to mean at least larger than the average of all the differences between first and last quartile percentages in the entire series of experiments. Since we had 8 subgroups of *O*s and 5 tests, there were in all 40 differences between first and last quartile percentages; the average size of these differences proved to be 7.95. The difference in the case of the E+ group quoted above was 9.1. Further, the percentages in the second and third quartiles ought to be consistent with the differences between the first and last quartile percentages; thus if the first quartile percentage is notably larger than the last quartile percentage, the second quartile percentage ought not to be notably smaller than the third, if the difference is to have significance. Finally, if a consistent tendency appears in one group, to conclude that it is due to the main characteristic of the group one must be able to find the opposite tendency in the opposite group: if the E+ *O*s have a tendency to quick reactions, this character obviously cannot be related to their emotionality in any simple way unless the E− *O*s show a tendency to slow reactions.

The following tendencies were indicated by figures that fulfilled these conditions:

(1) A tendency for extraverts to make quick reactions and for introverts to make slow reactions to a noise stimulus. Thirty-six percent of all extraverts were in the fastest quartile of the first ten reactions, 24% in the second quartile, 28% in the third quartile, and 12% in the slowest quartile; a difference of −24 between fastest and slowest quartiles. The difference between the second and third quartiles is in the wrong direction, but so small that it is negligible. Only 16.4% of introverts are in the fastest quartile; 21.8% are in the second quartile, 23.6% in the third quartile, and 38.2% in the slowest quartile. As the extraverts and introverts were our largest subgroups, this looks like a true difference.

(2) A tendency for the first 10 reaction-times of emotional *O*s to have larger average deviations than those of calm *O*s. Of the emotional *O*s 30.3% are in the most variable quartile, 27.3% in the second quartile, 21.2% in the third quartile, and 21.2% in the least variable quartile; a difference of −9.1 between the first and last quartiles. This is not large, but the figures for the calm group are more striking. Twenty percent of these *O*s are in the most

[4] An *O* often belonged in more than one subgroup.

variable quartile, 20% in the second, 20% in the third, and 40% in the least variable quartile; a difference of +20 between first and last quartiles.

(3) A tendency for timid *O*s to make fewer errors in tracing an outline star by mirror-drawing. Of the timid *O*s 37.04% are in the quartile making fewest errors, 29.63% in the second quartile, 11.1% in the third, and 22.2% in the quartile making most errors; a difference of −14.8. Of the fearless *O*s 21.2% are in the quartile making fewest errors, 15.1% in the second quartile, 33.3% in the third, and 30.3% in the quartile making most errors; a difference of +9.1 between first and last quartiles.

The following are less consistently indicated:

(1) A tendency for more timid than fearless *O*s to react very slowly in the first ten. The percentage of timid *O*s in the fastest quartile is 28.5; in the second quartile it is 14.3, in the third 17.9, and in the slowest quartile 39.2; a difference of +11.2. The percentage of fearless *O*s in the fastest quartile is 27.3; in the second quartile 33.3, in the third 24.2, and in the slowest quartile 15.1; a difference of −12.2 between first and last quartiles. The fact that the percentage of timid *O*s in both first and last quartiles is above 25 indicates that this group is here bimodal, some timid *O*s reacting especially quickly and others especially slowly.

(2) A tendency for more timid than fearless *O*s to experience rapid fluctuations of the outline cube. The percentage of timid *O*s in the quartile of most rapid fluctuations is 32.1; that in the second quartile 25, in the third quartile 25, and in the slowest quartile 17.8; a difference of −14.4. In the fastest quartile the percentage of fearless *O*s is only 17.6, but in the second quartile it rises to 29.4; in the third it is 26.5, and in the slowest quartile 26.5; a difference of +8.9 between first and last quartiles.

(3) A slight indication in the direction of McDougall's law that extraverts tend to slow, introverts to rapid fluctuations of the outline figure. For the extraverts the percentages from fastest to slowest quartiles are 20, 28, 20, 32, a difference of +12 between first and last quartiles; but the tendency is not maintained consistently through all four. The percentages for introverts, from fastest to slowest quartiles, are 29, 23.6, 23.6, 23.6; a difference of −5.4 between first and last quartiles, too small to be significant. The only reference[5] McDougall gives to his experimental work furnishes no information as to how he distinguished experimentally between extraverts and introverts and secured that measure of degree of introversion necessary to know "the position of the subject in the intro-extravert scale." Our *S*s, when we measured their degree of introversion numerically by the size of the ratio of introvert to extravert answers, showed no correlation between this degree and speed of cube fluctuations.

One of the questions in the Marston questionary refers to speed of movement, quick movement being taken as an extravert trait. Since our most trustworthy result indicated an extravert tendency to shorter average reaction-times, it seemed desirable to find whether the estimate of an *O*'s speed of move-

[5] M. Smith and W. McDougall, *The effects of alcohol and some other drugs during normal and fatigued conditions*, 1920. Extraversion and introversion are not alluded to in this paper.

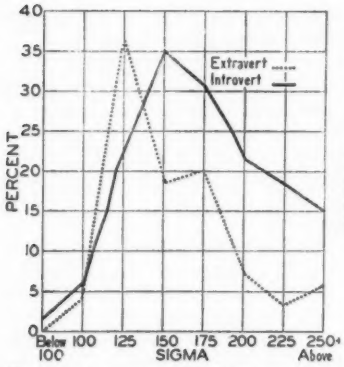


FIG. 1. AVERAGES OF FIRST 10 REACTION-TIMES FOR EXTRAVERT AND INTROVERT *Os*

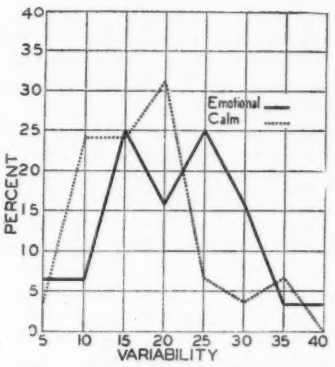


FIG. 2. VARIABILITY OF FIRST 10 REACTION-TIMES OF EMOTIONAL AND CALM *Os*

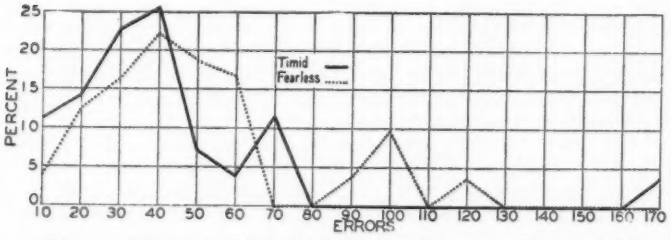


FIG. 3. ERRORS IN MIRROR-DRAWING OF TIMID AND FEARLESS *Os*

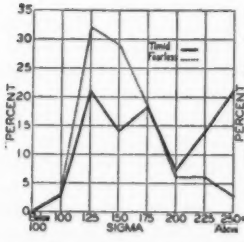


FIG. 4. AVERAGES OF FIRST 10 REACTION-TIMES FOR TIMID AND FEARLESS *Os*

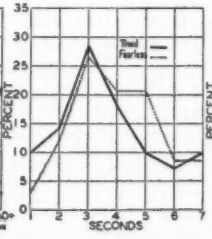


FIG. 5. AVERAGE LENGTH OF CUBE FLUCTUATIONS FOR TIMID AND FEARLESS *Os*

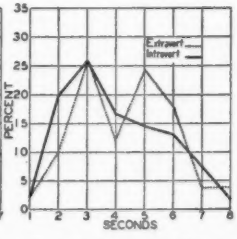


FIG. 6. AVERAGE LENGTH OF CUBE FLUCTUATIONS FOR EXTRAVERT AND INTROVERT *Os*

ment given in the questionary returns was borne out by the average speed of her reaction-time. If this were the case, then our experimental results on this point would mean merely that if you select a group on the basis of quick movement, the members of this group will move quickly. But we found that of those *O*s who were judged in the questionaries to move quickly only 43% had reaction-times below the median, while of those who were judged in the questionaries to move slowly only 40% had reaction-times above the median. There was thus no correlation between speed of movement as estimated in the questionaries and as displayed in simple reactions to noise, and *a priori* one would not expect with any confidence such a condition.

The accompanying graphs (Figs. 1–6) show the tendencies that appeared in our experimental results. As will be seen, the graphs are in terms not of quartiles but of 8 or more divisions of the scores, thus giving a more accurate representation of the distribution.

APPARATUS

A Duplex Marker

By Raymond Dodge and Roland C. Travis, Institute of Psychology, Yale University

This marker (Fig. 1) was designed by Dodge about 1908 for use with a projection chronograph. It writes two non-confusable curves, corresponding to two sets of experimental data, on a single line with a single writing point, and thus guarantees permanent alignment and conserves space.

It has been used to record time and reaction, stimulus and response, and two sets of reactions. It has recorded on various smoked surfaces, on plain paper with ink, and with appropriate optical additions on photographic paper and film. In all these situations and various others it has proved itself a reliable as well as a versatile laboratory instrument.

The essential feature of the instrument consists of a jointed recording lever of two segments—an armature segment A and a writing segment B. Each segment may be activated independently by its own electro-magnet (MA and MB) and rotated about an axis in the other segment. The proximal end of A is pivoted on an offset from the marker frame. The writing segment B is an aluminum bar and has a three-point support as follows: a point offset from the upper core of its magnet engages a cup a on the armature to which the lever is attached and provides a permanent point of rotation for all movements of the writing point. This may be combined as an axis either with b on the distal end of segment A or with either of the two stops at c, an offset from the lower core of its magnet or an adjustable stop attached to the frame of the marker.

If an electric current flows through MA the writing point is rotated to the left on the axis a-c. If a current flows through MB the writing point is rotated to the right on the axis a-b. By suitable manipulation of the adjustable stops the excursions in the two directions may be made different in amplitude and consequently non-confusable.

Two adjustable springs SA and SB furnish the required tension. The compression spring SA acts against the proximal end of B and serves both to maintain the articulation of the two segments at b and to force the armature segment against its stop. The helical extension spring SB holds segment B against the permanent axis point a and forces it against its stop c.

The construction makes segment B slightly more efficient. This segment is commonly used by us for tuningfork time-lines up to 100 d.v., and for other exacting service.

The marker may be muted by encircling each armature with a small elastic band where it strikes the magnet and stops. This device also decreases the instrumental latency. With optimum rubber cushions our records show a latency to current breaks below one sigma.

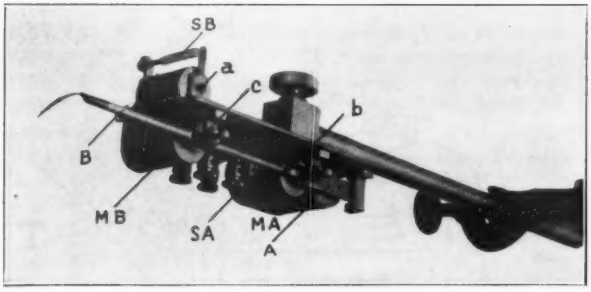
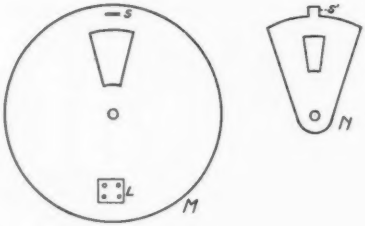
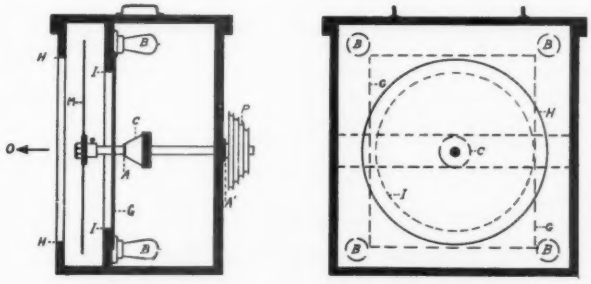


FIG. 1. A DUPLEX MARKER





Side View Front View

FIG. 2 AN IMPROVED MODEL OF MCDOUGALL'S AFTER-IMAGE APPARATUS

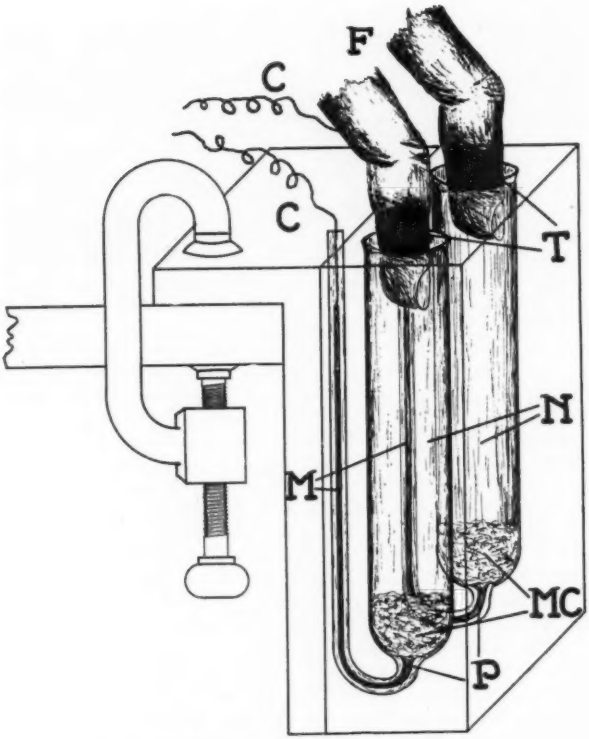


FIG. 3. A SIMPLE NON-POLARIZING ELECTRODE

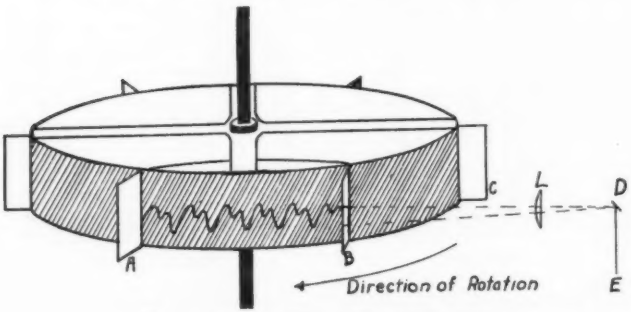


FIG. 4. A NEW PHONO-PROJECTOSCOPE

This duplex marker is already in use in several psychological laboratories. It is constructed by Mr. C. H. J. Newton of the Wesleyan shop. At present prices it costs about $35.00. It may be wound for either low or high voltage. We use a model that takes about 1.5 amperes current at 3 volts.

An Improved Model of McDougall's After-Image Apparatus

By W. N. Kellogg, Columbia University

In adapting McDougall's experiment on the travelling after-image to an advanced course in the psychological laboratory at Columbia, it was found necessary to alter considerably the design of the original apparatus. The model here presented may be regarded, therefore, as a practically new instrument.

McDougall sent light through a narrow aperture in a large rotating disk.[1] By this means the positive after-image appeared to the observer of the disk much like the tail of a comet, so that its stages and details could be conveniently studied. For our uses the apparatus had two serious defects: first, the large screen placed before the disk to prevent stray light from reaching the *O* seemed to call for a separate room to house the apparatus, and secondly, the location of McDougall's gas-light source required a strong support for the rotating shaft and this cast a troublesome shadow on the disk.

To eliminate the screen and the dark-room we used a black portable wooden box, Fig. 2. To remove the second defect and also to permit the installation of electrical connections the internal design was modified as appears in this figure. The rotating disk and its bearings and shaft are set in the square wooden box (ca. 70 cm. x 70 cm.), which is lined with white asbestos paper. The box has a circular opening at the front (Fig. 2, H–H) through which the observer (beyond "O" in the figure) sees the disk. Its two compartments are separated by a square piece of ⅛-in. opal glass (G) just behind the rotating disk (see Fig. 2, I–I). The shaft carrying the disk pierces this glass at the center and is sustained by bearings at A and A′ (side view). The pulley (P) behind the box is connected by a belt-drive to a variable speed 120-v motor. The light bulbs (B)—frosted 25-watt—are arranged to cast an equable light without shadow upon the opal glass. The intensity of light is regulated by the size and number of bulbs and by voltage. The box has protected ventilating openings in the removable top, and is supplied with handles. The disk is fastened to the shaft by means of a special flanged end-piece (Fig. 2), two 3-in. brass washers and a large nut.

In making the disk (see Fig. 2), we used 20-gauge galvanized sheet iron, painted black. The advantages over McDougall's "stout millboard" are obvious. Accessory sectors with various small apertures (*e.g.* N, Fig. 2) can be fitted over the larger opening in the main disk and the sector is held in place by the insertion of its projecting arm S′ into the slot S in the disk. A lead counter-weight, L, is bolted to the disk.

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[1] W. McDougall, The sensations excited by a single momentary stimulation of the eye, *Brit. J. Psychol.*, 1, 1904, 87-92.

A Simple Non-Polarizing Electrode

By Christian A. Ruckmick and Emily Patterson, University of Iowa

With the increasing use of electrical devices for measuring bodily resistances and bodily E.M.F., attention has been recently drawn toward dependable and fairly simple electrodes. Various types have been used and improvements suggested from time to time.[1] Over a period of years many of these types have been tried out in our laboratory and have been discarded in favor of a modified Ostwald type illustrated in the accompanying figure (Fig. 3).[2] When the proper precautions against battery-effect as outlined by Jeffress[3] are exercised we have a dependable and uniformly responsive contact with the body.

In the authors' judgment this electrode fulfills three desiderata that apply to electrodes in general:

(1) It must not vary the resistance to either ectosomatic or endosomatic current through variation (a) in applied external pressure, (b) in amount of sweat on dry electrodes, and (c) in the amount of supplied moisture contained in surfaces like leather, cloth, or kaolin paste.

(2) It must be fairly convenient in form and comfortable, otherwise distracting elements will be added. As in the use of standard plethysmographs and pneumographs, *S* must cease to become directly aware of the connection in the progress of the experiment. This is not the case with platinum hypodermic needles or with tightly strapped devices.

(3) It must not introduce variable electrical components either through battery-effects or through polarization.

The chief disadvantage of the electrode here described is that it can be used only with finger contacts. Some of the other electrodes, like the type mentioned by Lauer, are adaptable to other regions of bodily surface. Since our electrode can be clamped to a stand, chair, or table, it is adaptable to use with the body in standing, sitting, or prone position.

The accompanying illustration, Fig. 3, shows in outline a wooden container clamped to the edge of the chair. The form of chair that has served us best is one with a desk-arm at the left. In our experiment this leaves the right arm free for writing introspections or manipulating apparatus if necessary.

The two glass receptacles are identical; each one consists of a pyrex glass tube (N) 16 cm. long, 3 cm. in diam., at the bottom of which a smaller glass tube (M) approximately 8 mm. in diam., is fused and bent up in the form of a J. At the top it is fused again with the rim of the larger tube to insure stability

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[1] A. R. Lauer has given us an excellent review of the variety of electrodes which have been used in laboratory work. Cf. A new type of electrode for the galvanic skin reflex, *J. Exper. Psychol.*, 11, 1928, 248-251.

[2] In its original form it was suggested by Professor J. N. Pearce of the Chemistry Department in this University. It was described in detail by N. Bayley (A study of fear by means of the psychogalvanic technique, *Psychol. Monog.*, 38, 1928, no. 176, 8-11). The improved form is now made by the Robbins Company, Iowa City, Iowa, at an estimated cost of $2.50 per unit.

[3] L. A. Jeffress, Galvanic phenomena of the skin, *J. Exper. Psychol.*, 11, 1928, 130-144. (See especially 133f.).

against breakage. Where this smaller glass tube is fused at the bottom of the larger glass tube a platinum wire (P) has been fused into the neck, forming a solid mass with the glass and thus preventing the passage of liquid from the larger to the smaller tube. This wire projects a little way into each tube.

Into the larger tube (N) redistilled mercury is poured until it covers the platinum wire. On top of this, mercurous chloride (MC) is poured to the depth of about 3 cm. From this point almost to the top the tube is filled with 1% sodium chloride solution.[4] Into the smaller tube redistilled mercury is poured to a height of about 10 cm. from the bottom of the larger tube. Into this an amalgamated copper wire is inserted (C). It forms the electrical connection to the galvanometer and variable resistance units.

By experimenting with various types of contacts we have finally evolved the following technique. The skin of the first joint (F) of the first (index) and third fingers is carefully washed with 90% grain alcohol to remove any oil deposits from perspiration. Just above the crease of the joint one-fourth inch wide adhesive tape (T) is carefully wound so it forms a tight junction with the skin but not so tight that it will prevent normal circulation.[5] When this is neatly applied once or twice around the finger flexible collodion can be brushed on the surface of the tape to keep it from becoming dirty in the course of the day's work. With this arrangement the tape may be left on for several days at a time. Thus a constant area of the skin may be kept for a number of readings on the same *S*.

Before readings are taken *E* should carefully inspect the level of the liquid in the tubes so that it reaches well above the lower edge of the tape on the two fingers. It is advisable to insist that *S* touch the inside of the glass tube with the tips of the nails of the two fingers for the double purpose of keeping the fingers steady within the tube and also in order to prevent the area of the skin becoming reduced through contact with the inside of the glass. It is also necessary to keep *S*'s arm in a comfortable and relaxed position. Any strained position will manifest itself through variable galvanometric readings.

When the tubes are not in use they should be carefully corked to prevent evaporation and consequent concentration of the saline solution.

It will be seen from this description that direct effects of perspiration are eliminated although of course the possibility remains of changing the ionic condition of the liquid when the fingers are introduced. This in our experience, however, is negligible. The pressure is kept constant and the electrodes are very comfortable with *S*'s arm resting on a pillow placed on the arm of the chair. After repeated trials with different types of electrodes we have found that the type described above best meets our experimental requirements.

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[4]We have found this percentage satisfactory in our experiments. Some experimenters have been using as high as 20% saline solution; see Jeffress, *op. cit.*, 133.

[5]We have found this technique more desirable than the use of rubber finger cots mentioned by D. Wechsler and H. E. Jones (A study of emotional specificity, this JOURNAL, 40, 1928, 600-606). Usually undue constriction results from the use of these cots.

A New Phono-Projectoscope

By Milton Metfessel and Joseph Tiffin, University of Iowa

The commonest device for projecting sonorous wave-motion for visual inspection or demonstration has been the Koenig rotating mirror, in use by the physicist for over a half-century.[1] The apparatus herein described seems to us to be an improvement upon this convenient device. It consists of a horizontally rotating drum about 2 ft. in diam. and 7 in. in height. On the circumference of this drum are placed 6 projecting plates, each plate forming an angle of about 30° with the radius of the drum at that point. The plates are of the same height as the drum and project about 2½ in. outward. On each plate is glued a piece of white cardboard the same size as the plate.

The sound-wave (or any oscillatory motion which it is desired to project) is caused to actuate a beam of light by means of an optical lever. This beam of light is focused from one side of the apparatus upon one of the white cardboard plates from such an angle that the beam will continue to strike this plate through one-sixth of a revolution of the drum, *i.e.* until another plate has arrived to receive the beam. Retinal lag operates to enable the observer to see the waves which have been projected while the drum has revolved this distance. For continued observation of the wave it is only necessary to rotate the drum at such a speed that the time taken by any plate in traveling the distance between it and the next plate is less than the period of retinal lag. In practice we have found a speed of about 120 r.p.m. to be about right.

The operation of the device can be seen from the figure (Fig. 4). As the drum revolves, the beam E, reflected by the optical lever[2] whose fulcrum is at D, is carried by plate A until plate B arrives and intercepts it. Plate B then carries the beam until plate C arrives to take it, and so on. L is a convex lens whose focal length is about 3 ft. It is placed between the optical lever and the drum. The use of this lens of long focal length permits the fulcrum of the optical lever to be set far enough away from the drum so that the amplitude of the projected wave is practically the same over the distance where it is visibly projected. The circumference of the drum, shown by the shaded area in the figure, is painted black to cause a greater contrast between the projected beam and the background on which it is seen.

The advantage of this projectoscope over the old Koenig method is twofold. First, if any one of the Koenig mirrors does not form an angle of exactly 180° with the rotating axis, the wave will change its axis every time a mirror reflects it. The apparatus here described obviates this trouble simply, for the beam is always seen exactly where it strikes the white cardboard. The second advantage is that the light reflected from the optical lever is used much more economically than formerly in that none is lost in a second reflection. Using a Dorsey phonelescope in connection with this apparatus we are able to project sound-waves so clearly that they are easily seen by an entire class in broad daylight.

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[1] R. Koenig, *Phil. Mag.*, 45, 1873, 1-18, 105-114.

[2] A number of these are on the market, probably the most popular being the Dorsey phonelescope, manufactured by H. G. Dorsey, Washington, D. C. (Cf. *J. Opt. Soc. America & Rev. Scient. Instr.*, 6, 1922, 279.)

The Cinematoscope as a Memory Apparatus

By W. B. Pillsbury, University of Michigan

Miles and others have reported uses of the moving picture projector as a tachistoscope and related instruments. I have found it possible to use one as a demonstration memory apparatus, and it might be used for research. I use a 16 mm. amateur instrument. The syllables may be written in India ink on a transparent leader strip. It is advisable to write them on at least two, and better three or four successive exposure spaces, for one exposure does not give a clear impression. Better still, the syllable pairs for learning may be printed or made of pasted letters and then photographed. A turn or two should be given to each pair. Then the single test syllables may be taken one at a time.

In showing them the series of paired syllables may be cut from the film, the two ends of the film pasted together and if the exposure apparatus is turned by hand, the series can be given as many exposures as necessary to learn. When learning is complete, the single first syllables may be shown one by one and the number remembered determined. In a class, many of the laws of learning may be demonstrated.

The cost of one of the standard patterns is no more than is asked for a good memory machine, and if the syllables are printed by hand the cost of use is negligible. The many other uses of the instrument more than justify its purchase.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Psychologie Experimentale. By H. PIÉRON. Paris, Armand Colin, 1928. Pp. 215.

This short treatise is not, as its title might indicate, a textbook of experimental psychology, nor a laboratory manual. The author has already published, in collaboration with Ed. Toulouse, an experimental text. The present work is, as the author explains, a brief exposition of mental function, deduced from the well-established experimental findings of the past, and hence not differing radically from classical treatises.

The book is written simply, for popular reading, in an easy conversant style, and yet with scientific reserve and precision. In this respect it differs strikingly from the style of some of our recent American psychological literature for popular consumption with its lurid titles and 'cocky' phraseology, reminiscent of newspaper journalism. It fulfills the aim which the publisher has set for the collection of which it forms a part, namely to "popularize without vulgarizing."

In a short historical introduction, the author defends the proposition that "in reality experimental psychology was born of physics," mentioning Sauver as the discoverer of difference tones, LaHire of the flight of colors in the after-image, Herschell of the blindness of the fovea in feeble light, Bougeur of the psychophysical law which is attributed to Weber, and the astronomer Bessel of reaction-time. Physiology, however, receives credit in Weber, Helmholtz, etc, while pathology is represented in Binet, Janet, and Féré. Piéron evidently himself follows the physiological tradition, and the theoretical position of Ribot, in stressing behavior and bodily process.

The author's theoretical position is brought out in his brief discussion of the aims of psychology as a science. Characterizing the view of Wundt and Ribot, that psychology deals with subjective phenomena, as a "gigantic illusion," and declaring that "it is in this way that an entire domain of psychological studies, which are really concerned with particular forms of activity, of behavior, of characteristic modes of reaction, principally verbal, have been falsely considered as treating of phenomena of consciousness." He says, "It is certain, that, from my subjective viewpoint, everything is a conscious impression, whether I am dealing with physics, chemistry, or psychology; but from another person's viewpoint, the word green, or the word sensation has no more essentially subjective signification than the word cube or the word atom." The latter viewpoint he considers the scientific one, while "auto-psychology" cannot be considered scientifically valuable. "Science," he continues, "represents a body of *communicable* experiences, and cannot be interested in any thing which cannot become the object of collective experience, and social interactions."

How, then, does psychology differ from physiology? Merely in dealing with the integrated reactions of organisms, "as differentiated from the partial mechanisms, and limited systems of reaction" with which physiology concerns itself. Between the method of animal and child psychology and that of adult human psychology, "there is no essential difference. There is merely the greater facility of obtaining reactions, when one can use the more rich and flexible verbal instrument."

This point of view, far from leading the author to sympathize with such systems as those of Watson, whose theoretical position Piéron recognizes to have risen, like his own, from parallel studies on animals and humans, differs in the following striking manner from Behaviorism: it finds no reason for rejecting the vast body of studies which the author feels have merely sailed under false colors of subjectivism, when in reality they were objective all the while, and hence scientifically valid. How else, he points out, could the physicist, Sauvier, who was stone deaf, have been a pioneer in the study of musical tones? Again, the psychogalvanic reaction, and the registering of action currents, methods whose possibilities are far from being exhausted, indicate but do not permit a complete study of the nervous reactions such as are produced in the course of associative processes in the cerebral centers. It is the verbal reaction which renders these implicit reactions capable of objectification. Thus the image is an internal reaction following upon stimulation, which can be rendered explicit either by a verbal expression, as saying "image," by graphic representation, or by description.

Thus, in spite of his rejection of the method of "self analysis" as a scientific one, the author, by emphasizing the importance of the verbal reaction as an objective datum, both legitimatizes the older experimental findings in such fields as sensation, perception, etc., and paves the way for further studies of the same kind.

Now this point of view is fair enough, provided it does not blind the investigator to the fact that the data thus objectively arrived at need constant interpretation in terms of the much despised self analysis, before they have meaning.

Throughout the book—which is divided into six sections, dealing respectively with reaction in general; affective reactions; perceptive reactions; intellectual reactions; habit functioning, work and fatigue; and levels of mentality and development of types—the author preserves the dynamic terminology and point of view. Part I deals with the concept of reaction, and 'stimulus-response,' from its simplest form in inherited reflex and automatic behavior to its highly individualized and variable aspect produced through conditioning. "The concept of conditioned reflex is capital," says the author, "it being the phenomenon of associative transfer which forms the basis of all acquired tendencies." Instincts are combined reflexes, differing only in degree of complexity, not in a hypothetical purposiveness. Yet he stresses the affective aspect of instincts as contributing the driving force. His list of instincts is modest enough, each corresponding to some definite group of "needs" of the organism, as hunger, thirst, need to urinate, discharge semen, or evacuate the mammary glands. His doubtful ones are child raising and gregariousness, for which he fails to give any organic basis.

Volition is recognized, but is characteristic only of highly organized acts, not of the elementary processes which compose them. Thus volition does not ordinarily initiate but merely regulates the on-going activity. Higher integrations cannot be understood without recognizing the tremendous influence which society exercises on human behavior through language and conventions.

The treatment of perception and of the higher intellectual processes is conventional. We think in terms of words, and symbolic attitudes. "What does it mean to comprehend? To comprehend is in reality to adopt an adequate attitude, mental or exteriorized." Language being the necessary instrument of thought, it is doubtful whether animals advance beyond motor trial-and-error. Man, by the use of creative thought, economizes experience. Attention is defined as a concept which merely "expresses the existence of an 'interest' and of a correlative orientation of activity and attitudes, an orientation defined and unified in the sense that the organism is entirely subordinated to the direction imposed by the dominating interest." Certain individuals lack attentive capacity due to the feebleness of their interests or their lack of mobilizing energy. I see no justification for the author's dogmatic assertion that peripheral theories of attention "confound the feeling of being attentive with the attentive process itself" and that the cardiac, vascular, respiratory and muscular modifications are overflow phenomena without utility. This position, in advance of proof, is no longer justified, in view of the recent discovery that muscular tension increases the efficiency of mental processes.

The chapter on work and fatigue, and the discussion of effort are distinctly modern in tone, as are the discussion of growth of intelligence, personality types, and mental profiles. But in the author's treatment of race and sex differences, it strikes one as strange to hear it asserted unconditionally that "the masculine type possesses a more coherent logic, a more marked ability for invention and intellectual creation, etc., than the feminine;" and that, "between white, yellow, and black races there are differences in type of intelligence and character, which exist even when training has been identical."

Incidentally, the author shows an intimate knowledge of recent American psychological literature, and makes frequent reference to American authors, even though the rule of the book is to omit all references, in order to conserve space. The book is well done, and in the main expresses a sane and critical viewpoint.

University of Chicago ARTHUR G. BILLS

The Future of an Illusion. By SIGMUND FREUD. Translated by W. D. Robson-Scott. No. 15 of The International Psycho-Analytic Library, edited by Ernest Jones. The Hogarth Press, London, 1928. Pp. 98.

This atheistic essay possesses all the serenity, kindness, and temperate reasonableness of a valedictory, although the controversial nature of its subject has always been, for less well-balanced writers, a temptation to dialectic excess and fanaticism. In phrases marked by simplicity and reserve, and unmarred by a self-conscious expression of disdainful superiority, Freud argues first that religion is necessarily a body of dogmas that hamper the free exercise of our rational faculties, which are after all our only resources, and secondly that it

is a nightmare of untrained minds, an intellectual error unavoidable in the prescientific infancy of mankind, but to be outgrown as soon as possible and replaced by reason.

What path does he take to arrive at this conclusion? Two widely diffused human characteristics are responsible for the fact that the organization of culture can at present be maintained only by certain measures of coercion: that is to say, men are not naturally fond of work, and arguments are of no avail against their passions. Therefore, the problem of civilization has been to diminish the burden of sacrifices imposed on the instincts, to reconcile men to those that must necessarily remain, and to produce a compensation for these. All would be well if the leaders were people of superior insight who had attained the height of mastering their own instincts. But the danger exists that in order not to lose their influence they will yield to the masses more than these will yield to them. Indeed, Russian mysticism has come to the sublime conclusion that sin is indispensable for the full enjoyment of the blessings of divine grace, and therefore fundamentally pleasing to God. Also, that the greatest sinners make the greatest saints is an old doctrine. Hence, an acceptable form of guidance is not to be found in the ruling class.

What remains, then, is to create a majority that will have a proper respect for reason. Having experienced the benefits of culture early in life, they will feel it to be their very own possession and will be ready on its account to make the sacrifice in labor and renunciation of satisfaction of the instincts to the degree necessary for its preservation. They will be able to do without coercion and will differ little from their leaders. If no culture has so far produced this condition, it is that no culture has yet attempted to influence men in such a way. One must ask the question, then, whether and to what degree it would be possible for another cultural milieu to efface the two characteristics of human masses that make guidance so difficult. In other words, it becomes a problem concerning the extent of man's capacity for education. Thus Freud makes his transition from the economic to the psychological plane.

The entire problem is, then, what must be done to reconcile men to and recompense them for their unavoidable sacrifices on behalf of the preservation of civilization. Hitherto religion has found a solution in threats against disobedience and the promise of a later opportunity for the gratification of wishes, as a reward for obedience. Now the nature of this solution must be examined. In a manner somewhat suggestive of Tom Paine, Freud touches upon the literary problem of the authenticity of these promises as given in revealed religion, and of course rejects them as patently unauthentic. Then he examines the promises themselves in order to discover their psychological determinants. In order, he says, to preserve culture, with which mankind defends itself against the crushing supremacy of nature, man must renounce the free play of his instincts. But the desired wish-gratification which religion offers as recompense for this renunciation is only a typical illusion in that it is not the residue of experience or the final result of reflection, but rather a fulfilment of the oldest, strongest, and most insistent wishes of mankind. The secret of its strength is the strength of these wishes. It betokens a tremendous relief for the individual psyche. The part played by the wish in this error is very clear. The illusion need not necessarily be unrealizable or incompatible with reality, but to reason the

dogmas of religion are no assurance of the possibility of realizing the illusion. Nor is verification to be found in mysticism, for if the truth of religious doctrines is dependent on an inner experience which bears witness to that truth, what is one to make of the many people who do not have that rare experience? Of what significance is it to others that one has gained from a state of ecstacy, which has deeply moved him, an imperturbable conviction of the real truth of the doctrines of religion? There is no appeal beyond reason. Ignorance is ignorance, and no right to believe anything is derived from it.

It must not be thought that Freud regards these pronouncements as original or as aught more than steps necessary to the logical development of his argument. Indeed, the reader cannot but observe their strong resemblance to the scepticism of Hume. But where that timid philosopher avoided the consequences of his logic and compromised with conventional religion, Freud, adding a certain psychological foundation to the critique of his great predecessor, boldly follows the reasoning through. As the illusion of religion is created out of the material offered by the memories of the helplessness of the childhood of the individual and the childhood of the race, it cannot serve for relative maturity and should be resolutely discarded. Already culture has little to fear from the educated. In their case religious motives for civilized behavior would be unobtrusively replaced by other and secular ones; besides, the educated are themselves supporters of culture. But it is otherwise with the great mass of uneducated and suppressed, who have every reason to be enemies of civilization. So long as they do not discover that people no longer believe in God, all is well. But what will happen when they do discover it and accept the results of rational science, without having effected in themselves the changed conception toward the world which scientific thought induces in men? To prevent chaos, two courses are open: either the most rigorous suppression of these dangerous masses and the most careful exclusion of all opportunities for mental awakening, or a fundamental revision of the relation between culture and religion. That revision, and it is the only one of the two courses that is advisable, must take the form of a rejection of religion and replacement by science and reason. Thus religion as a universal obsessional neurosis of mankind would give way to a rational acceptance of the inevitable needs of society.

The only question remaining is whether reason and science are adequate substitutes for religion and capable of performing what is desired of them in teaching men to become reconciled to civilization. In view of the failure of religion in the past and the inescapable chaos that awaits its continued failure in the future, and in view of what science has already accomplished in this its infancy, Freud feels, despite admitted weaknesses in the argument for reason, that in it lies justification of hope for the future and that perhaps a treasure may be dug up that will enrich culture.

Cornell University ELLIS FREEMAN

La Psychologie Comparée. By MARIE GOLDSMITH. Paris, A. Costes, 1927. Pp. 354.

The successive and constant reactions of organisms to the diverse factors of the external environment is what the biologist means by life. From the simplist living substances up to the complex human organism there is but a gradation

in the number, the complexity, and the remoteness of factors which serve as stimuli, and as we go up the scale we find a corresponding increase in the complexity of responses and in their range. The present volume recognizes that, aside from various philosophical questions and persisting uncertainties regarding the presence and nature of conscious experiences throughout this scale, a great number of objective facts have been accumulated, especially by the behaviorists in America, constituting "an experimental work of capital importance." This is a branch of knowledge very little known yet in France, so the author attempts to sketch the main facts of the young science of animal psychology, to show how it presents its problems and what solutions have been suggested. The first chapter deals chiefly with amoeba and is followed by a long and rather full account of the tropisms and of Loeb's theory of tropisms. The theory of Loeb is treated sympathetically in the main, though inadequately in certain respects, and its limitations are indicated briefly, particularly as developed in the work of several experimenters. Jennings is given a separate chapter. The author considers the term "trial and error" unfortunate when applied to the lower organisms, because it seems to imply a certain degree of conscious striving for an end, which not even Jennings intends it to mean. The numerous experiments and the detailed observations of Jennings have served, we are told, to make more apparent than real the simplicity of the behavior of the lower organisms, but Jennings can hardly be said to have contributed a theory; "he does not propose any explanation of the facts observed." The simple protozoön makes numerous reactions which neither the Loeb nor the Jennings theory would lead one to expect. Loeb's work is valued highly as being effective in clearing away metaphysical explanations of behavior. Very good summaries are given of reflexes and instincts, and considerable space is devoted to experiments and observations refuting the ideas of infallibility and invariability of instincts in insects. There is no reflection in the book of recent discussions in America concerning the nature and existence of instincts; but the author does the better thing, she confines herself to the facts that bear on the behavior of unlearned acts. Instincts are considered to be only the functionings of innate structures making for the preservation of life, and the variabilities of instincts are held to have the same forms as have the morphological characters. Much attention is given to the origin of instincts, and the work of Pavlov on the transmission of conditioned reflex, which has now come to naught, is regarded as significant of a possible line of explanation of certain changes in successive generations. The author regards it as certain that in some form habits are inherited, drawing for evidence of this upon Darwin's writings on the domestication of animals and upon other studies of earlier days. No attention is paid to such recent contradictory work as that of Child and others. The work on 'memory' and the associative processes is traced through the scale of animal life, considerable attention being given to the experiments of American psychologists on these processes and on imitation, delayed reaction, multiple choice, etc. The points of view of the authors themselves are adhered to pretty closely, and a rather good statement of the leading contributions is made. The weakest point is probably the discussion of the possible mechanisms in the formation of habits, which neglects much of the later critical literature. The author, after considering the problem of

fixation of habits by pleasure, rightly gives up the hope of getting any light on the problem from the mere fact that pleasure is associated with certain physiological activities and pain with others. Such a position, she points out, commits one either to the point of view of psychophysical parallelism or to that of interaction. The fact that recency cannot be a selective agency in learning, because it could only tend to fix the errors produced at any time or at any stage in the learning, is explicitely recognized; but the difficulties of explaining how persisting erroneous acts are finally eliminated are not given their due attention. On the whole the book is well organized and is sure to interest intelligent readers in psychology from a genetic point of view which is most essential for the solution of problems in human behavior.

J. P.

Manual of Psychiatry. Edited by AARON J. ROSANOFF. Sixth edition, revised. New York, John Wiley & Sons, Inc. 1927. Pp. xvi, 697.

Every psychologist who has even occasional need for a guide to psychiatry must long ago have discovered the indispensable values in Rosanoff's *Manual.* Hence a new edition of that work is appealing. Naturally one looks first for the new material, and notes with pleasure a chapter on the psychoses associated with lethargic encephalitis. There is also one on pellagra effects. The material on personality has been brought together in a special chapter and there is a new chapter on the disorders of childhood. The Hoch-Amsden guide has been added and also tables of height-weight norms and the Inglis tables for the computation of intelligence quotients. To the presentation of association reactions there is added a list of juvenile reactions taken from the Woodrow and Lowell study. Aside from these and some other additions there is much rearrangement and the occasional inclusion of new material, although these changes are not sufficient to be seriously confusing to those who have familiarized themselves with the organization of the preceding edition. The index now includes definitions of technical terms. To the list of contributors has been added the name of J. Harold Williams.

In the seven years since the fifth edition there has been a voluminous addition to the literature of psychopathology and psychiatry. The reviewer therefore, turned quite naturally to the discovery of what in that literature had been deemed worthy of inclusion, of how the new had influenced the old, giving his attention of course to those sections and topics of most interest to psychologists. The chapter on hallucination has been expanded by the addition of a couple of paragraphs giving recognition to psychological interpretations, and it mentions especially the psychoanalytic, referring the reader to the chapter on psychoanalysis. The interested reader will discover, however, that the psychoanalytic theory of hallucination received no detailed presentation. The work of Jaensch and the eidetic school and its bearing on the interpretation of hallucination was not included. The chapter on mental deficiency has been much expanded and shows the influence of the newer literature, but the critique of the inheritance studies by Myerson is not included. To the chapter on epilepsy there has been added a paragraph on migraine with one case and one reference. The statistical presentations have been brought up to date and the presentation of hygienic measures considerably expanded. There is but one entirely new reference,

this being that on the ketogenic diet. The extensive literature which has come from the many studies by both the somatic and the psychogenic schools is omitted. The chapter on dementia praecox remains much as in the previous edition, with the same emphasis upon heredity as a causative factor. The term introversion and the literature which it represents found no place. Adolf Meyer is quoted at length but the psychoanalytic interpretations are omitted. In the presentation of the manic-depressive psychoses there appears a brief mention of "fixation of libido" as a possible pathogenic factor. References to endocrinological studies of this group do not appear. The tenor of the changes in the chapter on paranoia is indicated by the change of title from paranoia to paranoic conditions.

There is a new chapter on the theory of personality. It is written from a very practical and almost purely psychiatric point of view. The basis of classification is that which the editor has already made well known. His four forms are well presented by examples and descriptions, although it is notable that the epileptic personality is presented without a single reference to the many presentations of that personality by L. Pierce Clark. All but three of the references bear dates prior to 1920 and there is no mention of the groups or schools represented by Jung, Prince, and Hinkle. The literature on the relation of personality to body types is also omitted.

The chapter on psychoanalysis has been little altered. There are still the long quotations and the abstracts from the earlier literature of the subject. The later literature is not noted. Even at the close of the chapter where proper apologies are made for the impossibility of condensing psychoanalytic literature into so short a space and where the reader is referred to original sources for a better knowledge, the references given are nearly all prior to 1910, and only one is as late as 1918.

Mental disorders of childhood are presented in a new chapter which should prove serviceable, especially to physicians. To the psychologically trained reader the influence of the California group will be evident. The presentation of technique for the examination of aphasias still remains that of Meyer. Head is not mentioned.

In the mind of the reviewer the new edition does not manifest as thorough an examination and digest of the literature which has appeared since the publication of the fifth edition as the previous editions did of the literature extant. Either that or the appraisal of the later literature differs greatly from that made of it by the reviewer. But readers unfamiliar with the Rosanoff *Manual* must not interpret this as detracting from the value of the book as a whole, which the reviewer still believes to rank among the few most serviceable.

University of Oregon EDMUND S. CONKLIN

Psychologie de l'Art: Essai sur l'Activité Artistique. By HENRI DELACROIX. Paris, Félix Alcan, 1927. Pp. 481.

The author does not pretend that this book is a philosophy of art or even a psychology of art. He calls it *Psychologie de l'Art* because, as he says, the title is convenient and because it indicates his intentions.

In the first part, Delacroix discusses the most general conditions of esthetic activity. He approaches the world of art through the world of play. The

great themes of biological and social life are operative in the adult's play. Revery, which is a kind of play, opens to the dreamer "another world" where he feels more at ease. Whereas the player is almost entirely indifferent to his material, the artist begins by loving the material in and for itself, aside from what it aids him in expressing.

Art animates the universe; this, Delacroix believes, is what is true in the doctrine of *Einfühlung*. Animation is not, however, sufficient to define art, since it is just as much the condition of all intense life. Art is never contemplation of ideas. It is *actualisation d'esprit*. The pretended external *donnée* never comes in a pure state but always elaborated by the mind. There is no esthetic *donnée* without an artistic vision; and the vision of the artist is already a work of art. Art begins by creating its means of expression, its language, its symbols. According to Delacroix, there is at the foundation of art the same intellectual act found at the basis of language.

Delacroix finds nothing in bodily expressions which will qualify adequately esthetic feeling. Feeling seems to be more extensive than its organic expression. These organic states form only a *étape de relai*, a moment of realization of esthetic feeling.

Inspiration and execution should not be set in opposition. Inspiration may reappear in the course of execution. There are artists in whom execution is only a succession of acts of inspiration. Delacroix mentions four forms of artistic production: brusque, sudden actualization or inspiration; a slower form; the form of conscious and reflective work; and the form of habit and automatism.

In the second part of the book, Delacroix completes his analyses by some concrete examples borrowed from music, poetry, and painting. He believes that music has emerged, not from speech, but rather from the rhythm of all life and thought. Rhythm is primarily a fact of life, an organic phenomenon. Musical rhythm shows us the organization of time, space, and quantity by an active consciousness. It is order in time. The author contends that everything in music oscillates around a scale, that intervals are only fragments of scales, and that chords are only the superposition of the notes of a scale. He admits that preferred intervals seem to precede a regular scale. He gives a genetic interpretation of the difficult topic of consonance and dissonance. A dissonance, he says, is a potential consonance.

The first step in music is the creation of forms, such as the scale, rhythm, and harmonic consonance. Delacroix emphasizes again and again his view that art is essentially creation. It is a little disconcerting to read in the same volume: "Il n'y a donc pas d'abord une perception de données elementaires, puis une mise en forme de ces données. . . . Il n'y a que des ensembles organisés dans lesquels les parties reçoivent leur caractère de leur place et de leur fonction" (p. 286). In this connection he refers to Koffka and Köhler. Although form is important in music, there is, Delacroix says, no merely formal beauty, since a content is always involved in the form. Music consists in bringing to agreement the sensorial pleasure of sounds, the plastic and intellectual pleasure of sonorous forms, the affective pleasure of the feelings and of the inexpressible kind of life which there is behind them.

The verbal arts appeal to affective and logical, musical and plastic elements. As the morphological structure of language governs literature, its phonetic

structure governs the essential characteristics of verse. Delacroix accepts the general principle that all poetic expression results from association of ideas and form.

The constitutive elements of painting are design and form, and light and color. Form reveals the tangibility of the object. Light and color appeal to our feelings. The ideal of every painter is to bring about agreement between composition and conception, to adapt lines, chiaroscuro, and colors to the characteristic he wishes to prevail, to unite polychromatic, linear, psychological, tonal, and affective elements.

Delacroix believes that art and science are complementary; both are necessary for human life. Social evolution determines in part the development of art, but a "social theory" of art is insufficient. Introversion, narcissism, and split personality may be factors conditioning certain forms of art. Among all the varieties, what constitutes the common foundation of art is creation.

Delacroix's work will probably make a stronger appeal to philosophers than to American psychologists. His interest in experimental esthetics is somewhat limited. He gives many quotations from artists; this procedure is at times dangerous, as he tends to accept uncritically remarks by artists concerning their own work. The book is very readable, although it suffers occasionally from diffuseness.

The foot-notes are incomplete, and there is no index.

University of Illinois CHARLES WARREN FOX

Der Einfluss der negativen Phase auf soziales Verhalten und literarische Produktion pubertierender Mädchen. By HILDEGARD HETZER. Part I. Quellen und Studien zur Jugendkunde. Edited by Charlotte Bühler. Jena, Gustav Fischer, 1926. Pp. 43, 13 ill.

This study treats of *Ss* all of whom are taken from the proletarian social level. One general table contains names of 52 *Ss*, ranging in age from 6:1 to 13:3, all of whom are voluntary inmates of the Vienna municipal *Jugendhort XII*.

The problem consists in finding whether the writing urge found among pubescents of the better classes is also present in the proletariate. A chapter is inserted on the negative phase, with an appropriate table showing the time of the beginning, duration, and end of this phase. Seven *Ss* fill the table, than which a sufficient number could hardly have been less. One table illustrates the finding of literary productions in 8 of 12 *Ss*, noting the kind but not the frequency of products. The characteristic of the literature of this period is its introspective and emotional tone. Six appended plates of writings and drawings by the *Ss* contribute important first-hand data.

The second problem strives to find whether a change in social relations occurs at the beginning of puberty. A folded sheet containing a series of seven schematic figures accompanies the chapters on social behavior and leadership. In this group evolution, the author is the original center of leadership and authority. *Ss* coming into the negative stage relax their former attitude, attach themselves to another leader, who is more informed than themselves and who in this case is of a vicious nature and is ranged in opposition to the constituted

authority. With the attainment of pubescence, however, the *Ss* revert to their former social relations with their governess, and develop in addition special leaderships.

A final chapter is replete with incidental observations confirmatory of previous research on group coldness toward newcomers and intra-group contests for superiority. A social relation which might have merited consideration is that which crops out in the references to home and parents as found in the writings and drawings of the *Ss*. A bibliography completes the work.

Johns Hopkins University GREGORY J. SCHRAMM

Problems of Social Well-Being. By JAMES H. S. BOSSARD. New York and London, Harper & Bros., 1927. Pp. xvi, 654.

Social well-being is a state of society whose individuals are in a condition of well-being. Conditions for social and individual well-being are identical. It is probably easiest to make the meaning of well-being clear by stating what it is *not*, called by the author "ill-being." Formerly ill-being was treated under the three D's—defectives, dependents, and delinquents. The expression is too long and still it fails to be discriminating enough as to the insane, the feeble-minded, paupers, inebriates, the diseased, blind, and deaf. At least 15 percent of our population have lived in recent years at the poverty level; non-effectiveness due to ill health is between 5 and 6 percent; among those low in intelligence at least half a million are in need of institutional care. The author estimates that the annual cost to this country for care of ill-being is $2,000,000,000. Must this stupendous expenditure continue indefinitely? The author emphasizes the essential unity and interdependence of various social factors, the basic factors being, in his opinion, economic, physical, and psychological; and he does not stress the formulation of so-called sociological principles, being sceptical concerning the identification of such principles. The book contains a wealth of data from reliable sources on economic, medical, and other aspects of social well-being, which lie outside the field of our present interest—though by no means to be neglected by psychologists—and which therefore we must pass over. An interesting account of the progress of medicine is given, from the period of early superstitions to the modern view of germ infection and the methods of treatment and prevention, including an account of the modern public health movement. The causes of accidents are traced from their mechanical through their more strictly psychological factors—fatigue, excessive speed, professional daring, inexperience at the beginning of one's employment, youth, and lack of training. Finally, the effects of limitation of intelligence are briefly and very inadequately considered. This is partly accounted for by the statement that intelligence factors fail to get due recognition in the industrial safety movement. But for that very reason, would it not be well to marshall the facts relating to the reduction of accidents in certain fields of industry, especially in connection with the handling of electric cars and automobiles, by the elimination of workers of low intelligence and those having certain sensory defects, as well as by better placement according to specific qualifications? We are told that perhaps a majority of workers today believe that they are fore-ordained to die on a certain day, and that until the arrival

of the very hour that they are destined to go 'the Lord will protect them.' The removal of such attitudes must in large part be the work of a sound psychology.

Part III, covering 137 pages, is devoted entirely to mental hygiene in its relation to well-being. A full and, on the whole, very satisfactory treatment of mental deficiency, based on the work of leading psychologists in this line, is given in two of the three chapters of Part III. Leading investigations in the heredity of mental deficiency are considered, the author being careful to indicate that as yet many problems regarding the causes and heredity of feeble-mindedness are still unsolved. A short account of the historical development in the treatment of the mentally deficient is given, attention being paid to the relative costs as well as to effectiveness. Identification and diagnosis with registration of all recognized defectives is urged, as well as proper training and care. Such a scheme would reveal the families and individuals most likely to transmit feeble-mindedness, and therefore the ones most needing segregation. Steralization methods have failed to meet the expectations of the advocates of this plan, this being due in good part to public disapproval and hesitancy on the part of many informed persons because of uncertainties in diagnosis and prognosis. Such drastic action can apply only to very extreme cases at best. On January 1, 1923, there were 267,617 patients with mental diseases in hospitals for their care in this country, but this number would be much greater if those who are paroled and those who ought to be in hospitals were included. A chapter is devoted to mental diseases and one to problems of mental hygiene, the author following closely in these, as in other chapters more or less technical to different sciences, the several contributors cited, often quoting at length.

The book gives a broad and sound view of the several aspects of social well-being, a survey that is worth while to psychologists.

J. P.

Das soziale Verhalten von Mädchen während der Reifezeit. By LUCIA VECERKA. Part II. Quellen und Studien zur Jugendkunde. Edited by Charlotte Bühler. Jena, Gustav Fischer, 1926. Pp. 74.

The author assigns to Englishmen and Americans the pioneer laurels in this sphere of psychology. The study is a continuation of the advances made by Schjelderup-Ebbe, Katz, and Reininger. The investigator has had thirteen years experience with girls and has carried on this study in a systematic way for one year. The *Ss* range from 11 to 19 years, and are taken from schools at the secondary and normal-school level. They number in all 1421. The dominant method of inquiry was by questionary, which was supplemented by the author's observations.

A chapter on the basic relationships of the period, a second presenting the characteristics of a class as a group, and a third giving detailed consideration to each of these 26 class members, contribute much to the thoroughness of the work.

In a section on the arrangement or structure of the class as a group, the actual position of each member is ascertained in regard to leadership and class achievement. The data were acquired by an ingenious questionary. From

the answers returned it is clear that leadership is something distinct from class-achievement. Six curves and three tables illustrate these and other findings.

The chapter on the class as a group is based largely on observation, although reference is made to the previous questionary data. The rapid development of uniform and at times blind behavior was found to be characteristic of this group. The presence of suggestion was shown by laughter salvos and mass complaints. The group leaders are found in the middle section of the previous class achievement rank. No illustrations adorn the chapter.

The existence and function of subgroups within the class are considered in another section. The subgroup usually rotates around a gifted specialist. Contrary to boys' subgroups which exist for the exercise of special abilities, these exist for the purpose of discussion. They are organized regardless of the rank position of the individuals, who are variable in number. A curve illustrates these findings. A frequency of relationship which is clear from this curve, but which the author does not point out is the following: the upper third of the students contain four individuals who have made contacts with more than one group, the middle third has two such, while the lower part has none.

Relationships between individuals regardless of class limits are considered in chapters on fellowship, friendship, alliance, and solitarity. The results are derived from very large numbers of individuals by introspective questionaries and observations of the investigator.

The characteristic of fellowship is impersonalism in a common life. The consciousness of these qualities as distinct from the closer relations of friendship dawns at the age of 12:6, and is clearly known at 15:3. Higher social standing hastens this perception. Three frequency tables and four curves are included in this chapter, which treats of 1137 *Ss*.

The chapter on friendship is the most exhaustive and covers approximately twelve pages, illustrated by five tables, describing 1292 *Ss* ranging from 11:0 years to 18:11. The data were derived in answer to the question, "What I wish my friend to be." The pre-pubescents desire her to be "dear," the pubescents require her to be "very dear" and in quite another sense, while finally the adolescents expect "to love and be loved." Many other characteristic qualities are demanded, which can not be adequately included here.

The alliance or circle has, like the fellowship, many members, and, like the friendship it has a personal interest among the members. To that extent it may be considered a composite social state. It is generally consolidated around an extravagant idea, and appears distinctly between the ages of 12:6 and 14:6. This chapter involves 1210 *Ss*, ranging from 11 to 17 years. The questions were answered by the girls, mainly from memory, since there were no alliances in bloom at the time. A table and a curve accompany the chapter.

The difficulty of studying the retiring period of solitarity was overcome by the inquiry, "Do I prefer to be alone or in company? Why?" The positive and characteristic answer "to be alone with my thoughts" occurs between the ages of 13:6 and 17:3. The *Ss* who replied numbered 1236 and ranged in age from 11 to 19 years. Three tables and three curves complete the chapter. Obviously the pedagogical importance of the facts adduced is clear.

Johns Hopkins University GREGORY J. SCHRAMM

Principles of Musical Education. By J. L. MURSELL. Introduction by M. V. O'Shea. New York, The Macmillan Co., 1927. Pp. xvi, 300.

This is an attempt "to present a comprehensive study of the methods, the aims, and the agencies of musical education, based on our current scientific knowledge of the psychology of music." The author regards the outlook for music in America as extremely encouraging, but deplores the existence of too many special methods which result in a great waste of time and money for students who by force of circumstances are obliged to change teachers. He contends that whether the pupil is studying violin, piano, voice, or what not he should always be thought of as studying music in its more general aspects; that theoretical and practical music belong together. All musical education involves motor as well as auditory sensory experiences. Even the perception of pitch is not free from motor adjustments, and interval-recognition clearly implies them. *Takt* (the basic rhythm) and the various phrase rhythms are motor unities. One set of delicate muscular coördinations may beat the primary rhythm while others respond in various secondary rhythms. In syncopation the former of these oppose the latter. If the different rhythms become synchronized the syncopation effect vanishes, while if the melodic rhythm opposes too long or too strongly the *Takt*, the syncopation loses its character by the loss of the primary rhythm in the melodic flow. Accurate recording devices show that rhythms are not strictly temporal as was once supposed, but that accented beats are prolonged somewhat beyond their strictly temporal allowances. Indeed musicians who endeavor to follow strict time, and suppose that they are doing this, are shown thus to prolong the accents. Music that does not allow natural expression of different phrase unities is sensed as constrained, instead of being easy and excellent as when it is psychologically correct. The optimum tempo varies from individual to individual, as may be expected from differences in size, muscular coördinations, etc., and the tempo which feels natural to each musician is the one which best expresses his own intimate attitude toward the music. Hence to give a student the correct tempo one must first effect a change in his conception of the selection, must modify the whole motor setting which serves as its background.

The general practice by musicians of playing from memory, following the example of Liszt, who once astonished his audience by throwing away the score of a composition he was playing, is condemned by the author as a serious strain on the performer and as the continuance of a mere fashion "that constitutes a really terrifying ordeal without any good reason being evident." The performer could "save himself very much toil, anxiety, and nervous strain by having the score open before him, for an occasional, reassuring glance." The field of experimental work on musical memory is pointed out as a rich field for psychological study, one from which as yet only meager results have been harvested. The Seashore and certain German tests are discussed and are cited as yielding thus far only certain negative values, *i.e.* discouraging persons without the proper ability from entering music; but the author is hopeful as to the possibility of future developments of a more positive nature. Valuable discussions of musical training and of auxiliary agencies for musical education, stages in musical development, etc. will be found by those practically engaged

in musical education. It should be said that the author is himself a musician of psychological training. Questions and exercises are given at the end of the book for each chapter, and a bibliography of four and one-half pages, but not quite up-to-date so far as American developments are concerned, is added.

The treatment of musical training from the teacher's standpoint seems to the reviewer rather sound psychologically though it often assumes a somewhat dogmatic tone, going beyond what is yet scientifically established. While this book presents no new data on psychological aspects of acoustics, it is extremely valuable as suggestive of fields for further investigation.

J. P.

La pensée. D'après les Recherches Expérimentales de H. J. Watt, de Messer et de Bühler. By ALBERT BURLOUD. Paris, Félix Alcan, 1927. Pp. 192.

This work gives an extended review of the various publications by the men mentioned in the title bearing upon the nature of thought and related processes, with criticisms of them. The original work needs no further comment. Burloud accepts the method as valid against the criticism of Wundt and others.

The results are discussed under three main heads: Is there imageless thought? What is thought as a fact of consciousness and of what elements is it constituted? What is thought as activity? The first question is answered rather more in the negative than in the affirmative. The author concludes that while we may have cases in which we are aware of thinking but can find no images, still the relations, intentions, attitudes and what not, that are not themselves imaged would not arise in default of images. They may serve to give to the image the character of thought, but the images are implied.

The elements of thought, it is asserted, are not clearly distinguished or logically classified by the Würzburg school. They seem to reduce, on the one hand, to the tasks, intentions, or what not, that give direction to the course of thinking, and on the other hand they reduce to 'feelings' of one type or another. Task or direction is not necessarily conscious. If we accept it as an element of thought it can be regarded indifferently as attitude or intention. It is at once a scheme or plan and a directing force in consciousness. As feeling, the somewhat trite suggestion is made that it is necessary to distinguish the affective sentiments and intellectual sentiments. The awareness of attitudes, etc., fall in the latter class.

The nearest approach to an answer to the question of what constitutes the active force in thinking is found in Watt's discussions of the task in its various applications. Even to this question Watt does not give a specific answer. Task is a dynamic effect of consciousness itself. It is not shown what relation if any it has to physical processes or in what sense it is dynamic.

The defects grow from the virtues of the works, that they seek an empirical examination of the most difficult problems of psychology. They show that observation needs to be supplemented by thinking, the study of the completed process by consideration of its evolution. The book gives a good summary of the work of the school, although the criticism strikes one as on the whole superficial.

University of Michigan W. B. PILLSBURY

An Experimental Study of the Mental Processes Involved in Judgment. By B. P. STEVANOVIĆ. London, Cambridge Univ. Press, Brit. J. Psychol. Mon. Suppl., 12, 1927. Pp. 138.

This is an analytical study of the mental processes involved in judgment. The first part is preliminary and reports the learning of thirty-five pictures by 5 *Ss*. The pictures were divided into seven sets, each set of five pictures having a characteristic color and a meaningless name, as bogel, pimef, etc. printed above the pictures. Each *S* learned the pictures and their names "sufficiently well to know them as material for the later 'completion experiments,' and not quite so well as to make the completion too easy and automatic." This criterion gives no assurance that all *Ss* learned the pictures equally well, a fact which may have produced many of the individual differences found later on in the study. The second part of the experiment consisted in completing eight different kinds of sentences in the form of propositions, as "The first bogel is ....." "Some pimefs are ....." The *Ss* completed each sentence by memory as quickly as possible, while *E* recorded reaction-time and galvanic readings for every judgment. Two sets of introspections were taken, one with learning and one with the completion of the sentences. Analysis of the former introspections shows that the pictures go through about eight different stages in acquiring their final meanings. The sentences when completed were classified into individual, particular, universal, and general affirmative, and into universal, particular, and individual negative, according to the kind of judgments involved. The second class of introspections showed that the subjects of negative and particular judgments tend to be present as imageless knowledge more frequently than those of affimatives and universals. "A character obtained by associative reproductive activity is not accepted without verification." In general affirmative judgments tend to involve analytic processes while negative judgments tend to be synthetic. Changes in bodily resistance seem to accompany 'conation' but not 'volition.'

Unfortunately the results are couched in terms and logical categories which can hardly be non-influential in their determination, and interpretations are in terms of a special point of view not very common in America. The writer seems to regard 'conation' and 'volition' as arbitrary influences rather than aspects of behavior to be explained. Spearman's neogenetic principles are considered as ultimates, and the use of these conceptions complicate and vitiate results and conclusions, making their objective use by psychologists impossible. Because of this, many of the author's explanations seem to the reviewer like saying that something happens because it happens. The experiments, however, have several suggestive and interesting aspects.

Peabody College C. W. TELFORD

Applications of Clinical Psychology in Hawaii. By MARJORIE E. BABCOCK. Honolulu, Mercantile Press, 1927. Pp. 82.

This initial Research Publication of the University of Hawaii, written as a master of arts thesis, is a very interesting piece of work. The first part of it consists in an ethnographic, social and economic bird's-eye view of this polyglot territory. There follows a concise but fairly comprehensive review of the studies of previous workers in clinical psychology, in comparing racial groups.

In adapting the Vineland revision of the Binet test to special local conditions the author was compelled to shorten the Scale, using four tests instead of six for each year, allowing three months for each test up to the 12-year level. In years 12, 14, and 16, four tests were used with six months credit for each. Those tests immediately dependent on language were eliminated, as far as possible. It was found that the oriental children, especially, would not make any attempt to respond to the vocabulary, comprehension, and sentence repetition tests. The author feels that these Binet tests enabled her to measure rather accurately, the school proficiency. In all of her work she accepted 14 years as the average adult level.

All of her cases were, in addition, examined by the Porteus maze test. She feels that these tests afforded her a fair index of the subject's industrial trainability, planning capacity and prudence necessary to social adjustments. She emphasizes that the maze test has a negative value, in so far as failure is more significant than success. The literature on the Porteus maze test is reviewed, perhaps with too friendly a bias. The fifth chapter of her work is devoted to her own results in testing 760 heterogeneous cases, representing eleven nationality groups, varying from three to thirty-five years of age. The Porto Rican group was found to occupy the most serious position from a clinical standpoint. The average Binet I.Q. among them was 64 and the Porteus maze I.Q. 70. Generally speaking the Japanese or Chinese cases referred to the clinic average about 15 I.Q. points lower than the normal Japanese and Chinese population. The author concluded that the same probably applies to the Porto Ricans. The respective average Binet and Porteus maze scores for some of the nationality groups were: Portuguese 71, 73; Filipino 71, 83; Hawaiian 69, 84; Chinese 74, 76; Japanese 75, 84.

The sixth chapter of her work relates case histories. Although they afford some idea of the great diversity of her material, they fall short of the high standard of the preceding chapters. One feels that the author's ready tendency toward making generalizations for large racial groups on the basis of the samples (many of them quite small) which she had the opportunity to examine, is rather dangerous. In the case histories such words as "emotional" and "hysterical fit" are rather loosely used.

This little work impresses the reviewer as a real contribution to the study of comparative racial psychology.

Boston Psychopathic Hospital MANFRED S. GUTTMACHER

Sex Differences in the Growth of American School Children. By E. A. LINCOLN. Baltimore, Warwick & York, Inc., 1927. Pp. ix, 189.

This book is a modified and expanded doctorate thesis. Regarding its contents the author says, "I have tried to find the results of the most important of the previous investigations, and to assemble them, in such a way as to show in the clearest manner possible what sex differences exist and how significant they may be." After a brief historical discussion he presents his data in 148 tables with the necessary explanations and interpretations.

The data presented show large and significant differences in the physical growth of the sexes. Likewise in school accomplishment the results of standard

tests show the girls to be definitely superior to the boys in nearly all school subjects, the exceptions being history and arithmetical reasoning wherein boys show a rather uncertain superiority.

For general intelligence, however, the case is different. Some types of test give a real advantage to girls and some to boys. In the process of totaling results such advantages are obscured. As might well be expected, therefore, the author finds no significant sex differences in intelligence or its rate of development. The author's attitude towards the difficulty in weighting tests, which is crucial for his problem, is not altogether clear. After calling attention to the situation he remarks, "this means that the maker of an intelligence examination should be careful that he does not weight his instrument in favor of one sex or the other." Does he mean that such freedom from bias in favor of either sex is to be attained by a mere juggling of weights? Such a procedure would result only in hiding any sex differences which may exist. If, as may well be, one sex matures mentally more rapidly than the other, tests standardized in this manner will effectively obscure this difference and rate that sex lower than the other at maturity. But perhaps the author's word of caution is a call for the use of only such test items as lie equally within the interest and experience of both sexes in studies of sex differences. If so, one would think that this principle might well have been utilized in selecting and evaluating relevant results of his own and previous studies.

The apparent relative variability of the sexes is affected by the same inadequacy of the tests, as the author recognizes. Also another difficulty arises at this point. Lincoln finds, for example, that with respect to physiological maturity the girls are more variable below the 13.5 year age group, and the boys are more variable thereafter. To account for this and similar comparisons in other traits he suggests that "variability is a function of maturity." If so, why attempt to compare the variability of two groups which are at different levels of maturity in the trait compared? This difficulty might well be obviated by calculating variability from the central tendency of all ages at which the trait in question actually comes to maturity.

Kansas State College J. C. PETERSON

Freudian Essays on Religion and Science. By CAVENDISH MOXON. Boston, The Gorham Press, 1926. Pp. 133.

This book is a collection of reprints of the author's Freudian interpretations of divers subjects but more especially religion and religious interests and experiences.

Religion is considered as a "fear" which by the "regression of the libido to primary narcissistic and parental fixation points" has "driven men from erotic, intellectual and moral manhood." Christian creeds, more specifically the Nicene, may be considered as "compromise formations" appealing to unconscious erotic needs which should be superseded by "more adequate embodiments of human ideals." Citing Maeterlinck's interest in immortality as an example of the artist's creative desire, the author finds it to be "a self-love of dominance" and is, in fact, an imperfect rationalization or a necessary symbol of his unconscious desires dominating "the thoughts in his conscious creeds." The

appeal of Paul of Tarsus lay in his epileptic personality. Mystical and hysterical dream-states alike, are "symbolically mediated indentification with the mother —a narcissistic and homosexual activity" which in the case of the mystic, because of his abhorrence of sex intercourse, allows him his pretense of sex abstinence.

In the sixth chapter Moxon asserts that human fellowship is narcotic in its loosing of primitive impulses and its inhibiting of intellectual activity. The following chapters treat Freudian Criminology, Couéism, and the popular resistence toward the enlightenment of children on sex topics. There are included a psychoanalytical survey of the development of the libido of Frederick Nietzsche, and a critical essay on Freud's death instinct and Rank's libido theory.

Throughout the volume the author has given ample evidence of his faith in Freudian interpretations and methods and of his interest in Rank's "epoch making work on the Trauma of Birth" which has given a "psycho-biological foundation for Freud's findings." He believes, as stated in the preface, that these discoveries, when based on personal experience, give "a degree of control and direction of impulse beyond any other technical means hitherto discovered by man." The many dogmatic statements may be attributed to the intensity of this faith. The volume should be of interest to those who study religion and religious experiences from the psychological aspect as well as to the psychologist of more general interests.

Boston Psychopathic Hospital C. H. KIMBERLEY

How to Succeed in College. By W. F. BOOK. Baltimore, Warwick & York, Inc., 1927. Pp. 192.

This volume presents the results of an investigation of the capacities and adjustments of college students, and of some efforts to improve their attitudes and methods of work. Data were secured by means of an intelligence test, two reading tests, a questionary and a weekly time table filled out by freshmen under the supervision of their instructors during a regular class period. Results are reported for more than 3400 students, over 3000 of whom were freshmen.

Although girls are found to have less endurance than boys, the average time spent in study per assignment is 2.30 hours for girls as compared with 2.11 hours for boys. It should be noted, however, that the sole criterion of endurance consisted in students' answers to the question: "How long can you continue to work on a difficult problem before you get too tired to do efficient work?" Students who do some work for self-support make slightly better academic records than those who do no such work, although their average intelligence-test score is a little lower.

The records of students who were given a course in "How to Study" are compared with those of other comparable students. The average number of credit points earned by 43 freshmen enrolled in this course exceeded that of the entire freshmen class by 30 percent during the first and by 49 percent during the second semester, though their average intelligence rating was not superior to that of the entire class.

Special training in reading was given in the "How to Study" classes. This training was preceded and followed by tests of reading ability. Book reports an improvement of 102 percent in ability to read as a result of such training. One feels a little uneasy about this flattering result, however, when it is observed that calculations of reading ability were based on the time required to read a standardized assignment, each student reporting his own time.

The volume contains much information which should be both interesting and useful to college administrators, instructors, and students. To the casual reader its value may be somewhat impaired by an occasional show of disregard for the grammatical conventions.

Kansas State College J. C. PETERSON

The Psychology of Personality. By ENGLISH BAGBY. New York, Henry Holt & Co., 1928. Pp. viii, 236.

This volume deals with common emotional disorders. Those who are interested in the application of psychology to the broader problems of human conduct will certainly wish to read it.

The psychology of personality as it is treated in this volume involves for the most part a consideration of persistent emotional processes. As fundamental to the explanation of these processes in the individual, two concepts are used. The *activity level* of an individual varies from the rapid energy expenditure of excitement to the slow energy expenditure of depression. The internal conditions which at least in part alter the activity level of the individual and determine the intensity of reaction to external stimulations are characterized as *tensions.* It is pointed out that tensions may have various physiological causes. The notion of tension, however, has proved itself valuable in understanding and treating abnormal personality traits.

The conditioning of emotional reactions, the recall of repressed thoughts, worry, defense reactions, inferiority complexes, rationalization, day dreaming, and hysterical traits, are all considered and illustrated by a wealth of pertinent and interesting case material.

The general principles of treatment are clearly outlined. The basic clinical procedure of the book may possibly best be suggested by characterizing it as a combination of the behavioristic and the psychoanalytic methods. In order to secure coherence certain materials that might have been considered in a book such as the one under review are omitted. For example, the techniques for the detection of the repressed ideas are said to be the association method, dream interpretation, and direct questioning, but the technique of hypnotism is not explained. Even in presenting Dr. Morton Prince's "Bell Tower Case" the detection of the suppressed memory is attributed to a "special technique," but automatic writing is not mentioned. The last chapter contains a number of excellent cases chosen to illustrate the general thesis of the book. As the author himself well says, there has been an undoubted tendency in recent years to neglect purely descriptive studies in psychology. In this volume much has been done to make clear some of those phenomena of mental life which can be understood only when the everyday life of the individual is observed over long periods of time.

Brown University LEONARD CARMICHAEL

Educational Psychology. By DANIEL STARCH. Revised Edition. New York, The Macmillan Co., 1927. Pp. ix, 568.

No change has been made in general outline and point of view, except that a chapter on mental hygiene has been added. Some of the other chapters have been brought up to date. The new chapter emphasizes the need of establishing in the first seven years of the child's life proper habits, tastes, and attitudes, and thus of laying the foundation of his "life education" even in advance of his scholastic training in reading and writing. "Conflict," which has acquired a rather technical meaning in abnormal psychology, is used here as characteristic of "the interaction of a child with its environment." The importance of normal, vigorous, and direct attack on natural problems is emphasized, as well as the careful study and direction of the child to safeguard him against the formation in this early period of inferiority complexes and their consequent compensations. With the exception of the new material included from the recent classical investigation and other studies on transfer in school subjects, and that from the Chicago studies of eye-movements in reading, the added material is chiefly of educational interest only, relating to the teaching of specific school subjects. The chapter on learning remains unchanged and inadequate; and in view of the recent discussions of instinctive behavior, one is somewhat surprised to read that "the present conception of instincts is to regard them as specific responses with inherited neural mechanisms which will be set into action by specific stimuli or situations." The overlappings of various abilites in successive grades are still given as determined by a single measurement without any corrections according to recently developed technique, and the student is nowhere warned against the error of such procedure. Other important omissions are made, particularly those pertaining to establishing a sound psychological basis. Is it not about as simple, and much the better policy in the long run, to start out with the proper scientific precautions and methods? Many of the data now given in educational psychology texts on the teaching of school subjects seem to the reader to be simply education, and not to belong to psychology at all. The book contains a valuable summary of such work, brought up to date, so far as the more important aspects are concerned.

J. P.

An Introduction to Psychology. By JOHN J. B. MORGAN and A. R. GILLILAND. New York, The Macmillan Co., 1927. Pp. ix, 319.

The authors of this text offer the proposition that psychology has at last reached a stage where it may be added to the high school curriculum. To this end, they have designed their book "especially for the younger student;" they have dealt with material "specific enough to be understood by the more youthful intelligence;" they have omitted "any disputed material;" and they "have tried to avoid all possible confusion by means of simplification, examples, and questions of concrete nature." A laudable attempt from a pedagogical point of view if not strictly scientific and psychological! Examination of the material in the body of the book reveals nothing new in the way of fact or of form of presentation. Perhaps the "younger student" may have his interest quickened by the promise to tell him "why and how we study psychology." But that he

will be enlightened by the answers we are not so sure. Certainly the concrete material that is supposed to be requisite for his understanding is not present in the opening chapter where the methodology, the logic the postulates of science and of psychology are discussed. The effect of concreteness is striven for by means of illustrations, italics, and reference to the game of baseball. Whether the purpose of the book will prove of pedagogical value can be settled only by trial. The reviewer does not feel hopeful. Even though they designed the book for relatively uncritical readers, the authors should have avoided the numerous errors and inaccuracies that appear throughout it. For example, in Fig. 18 the blind spot and the foveal region are represented as coincident; Fig. 24 is not a "Color Pyramid" and is not so named by the author to whom they refer; the directions on p. 121 for locating warm spots indicate a temperature that is too high; the illusions of Figs. 43 and 44 are not so simple as their explanations and the slightly curious student will so discover by turning them on their sides. If accuracy must be sacrificed to make the material comprehensible to "youthful intelligences" the introduction to psychology might better be postponed until greater maturity is reached.

Hobart College FOREST LEE DIMMICK

La Realité de l'Esprit: Essais de Sociologie Subjective. By D. DRAGHICESCO. Preface by Lévy-Bruhl. Paris, Félix Alcan, 1928. Pp. ix, 276.

Up to a certain point this volume of Draghicesco appears to mirror the present day circumstances of American psychological science. In the view of this author the development of psychology has reached a crisis. A radical change of view is necessary. But in contrast with the American situation, in which the changes proposed are the extrusion from psychology of mysterious psychic processes in favor of natural science materials, Draghicesco insists upon the reality of the spiritual. Instead of making psychology more and more biological, which is the American tendency, he wants to achieve quite the opposite.

Draghicesco agrees with Kostyleff who thinks introspective psychology, even of the experimental type, has failed. But beyond this point there is violent disagreement between them. While the latter believes that the failure of introspective psychology points to Russian reflexology, the former insists that psychology must head at once and with full force toward subjectivistic sociology. The reality of the spiritual life according to him is only to be found in the interrelations of men in society.

This latter doctrine is the familiar concept of spiritualistic social psychology. Those who uphold this conception assume that since language, which is the expression of thought, could not exist except by virtue of social intercourse, so thought likewise depends upon social happenings. This conception is of course connected with the notion that thought is dependent upon expression. Such spirituality as emerges from social interaction, though not dependent upon the nature and functioning of the physiological organism, is still thought of as thoroughly objective.

So far is this from present day American thinking, where social psychology itself tends toward the individualistic and even toward the physiological, that

a basis for criticism is difficult to attain. When we add that Draghicesco regards this doctrine as having its inevitable culmination in religion it seems far removed from psychology as a technical discipline within the field of the natural sciences.

Indiana University J. R. KANTOR

A Sociological Philosophy of Education. By R. L. FINNEY. Introduction by W. C. Bagley. New York, The Macmillan Co., 1928. Pp. xi, 563.

This is a theoretical discussion of education from the sociological point of view. Its main interest to the psychologist is the uncritical use of terms with which he has to deal. "Social mind" is used "to mean that body of mental content which is held in common by members of the group, and through the use of which group action is made possible." The obscurity of this social mind is only increased when we read that in social readjustments series of acts occur "in the behavior of the social mind." Much is attributed to imitation. The real processes in learning are much obscured, and imitation is made a general formula for most uniformities in conduct. Psychologists will be particularly interested in the chapters on "The Blight of Extraneous Motivation," "Followership and the Duller Intellects," and "Educational versus Economic Determinism." The author accepts the general results of psychological tests, but holds that the lower mentalities act mechanically and in the main largely like the higher mentalities, through imitation and suggestion effected by means of catch phrases. Thus the reasoned behavior of the intelligent individuals is similar, generally speaking, to the rote behavior of the dullards. It is hopeless to teach the latter to think for themselves, so the intelligent must think for them and "drill the results, memoritor, into their synapses. For the dullard it is that or nothing." The psychologist who reads in this book of the various "insights of social psychology," a phrase that occurs frequently, cannot but sense the danger of the "psychologizing" of the sociologists. When it becomes so easy to 'explain' the various complex forms of social behavior merely by the superficial use of psychological terms, it is certain that not only 'dullards,' but many misguided students, will fail to see the need of delving experimentally by more laborious methods into the problems of psychology. The plausibility of many interpretations made by such writers only increases the deception, and delays the progress of real science. J. P.

The Psychology of Play Activities. By H. C. LEHMAN and P. A. WITTY. New York, A. S. Barnes & Co., 1927. Pp. xviii, 242.

The authors define play, with certain reservations, as the activities in which the individual engages "just because he wants to."

Several thousand children in the elementary schools of Kansas City, Missouri, were given check-list questionaries concerning the "play" activities in which they had engaged during the "past week." Data on the National or Terman intelligence test scores of these children were secured. Perhaps the most noteworthy finding is that "with increase in mental age, there was a tendency for the children to engage in smaller numbers of plays and games of a social character."

Also of interest are the following conclusions: "The data give evidence of a consistent tendency for the dull or retarded school child to evince an unusual interest in games in which others take part. . . . The dull or retarded child avoids certain individual activities."

Special studies were made of a group of "gifted" children; of the play of negro children; of the sex differences in play behavior; of the ages at which certain forms of games and play appeared, reached their maxima, and waned; and so on.

This study has been carefully and intelligently handled. It is of the utmost importance to anyone working in the field of child psychology, as it is the most complete and thorough treatment of the play behavior of children that has thus far been reported.

Harvard University DONALD SNEDDEN

The Social Sciences and their Interrelations. Edited by W. M. OGBURN and ALEXANDER GOLDENWEISER. New York, Houghton Mifflin Co., 1927. Pp. viii, 506.

The volume consists of 34 articles on the interrelations of some of the social sciences, and their relations to psychology and statistics; contributed by many specialists in the various fields. The work brings out clearly some of the divergent points of view of students primarily concerned with anthropology, economics, history, political science, or sociology. The optimism for the chosen field and the critical attitude towards outsiders are very apparent. This method of placing departmentalized attitudes side by side is very encouraging, and the book should be widely read by social scientists. There are many references to the basic subject of social psychology, some sympathetic and some critical, but all worth while. Psychologists will find the following articles interesting and also stimulating: "Political Science and Psychology" by Floyd H. Allport, "Sociology and Psychology" by L. L. Bernard, "History and Psychology" by Wilson D. Wallis, "Economics and Psychology" by Z. Clark Dickinson, "Anthropology and Psychology" by Alexander Goldenweiser, and "The Social Sciences and the Natural Sciences" by Morris R. Cohen. Some of the writers it must be said show an astonishing unfamiliarity with modern psychology.

University of Rochester HULSEY CASON

The Mental Life. By CHRISTIAN A. RUCKMICK. New York, Longmans, Green & Co., 1928. Pp. ix, 253.

In the first paragraph of this book the author tells us that Descartes wrote the Magna Charta for psychology. Throughout the volume an attempt is made to show that psychology is the science of experience and that it is not a branch of biology or physics. There is virtually no discussion of the nervous system or the response mechanism in the book save for occasional references to the receptor end-organs.

Sensory experience is treated concisely. It is presented in such a way as to be both clear and interesting to the elementary student. Excellent drawings are given of the diagrams which have been prepared to show the relationship of sensory qualities. The visual double pyramid, the tonal bell, the taste

tetrahedron, the smell pyramid, and the touch pyramid, are all clearly presented and explained. In the opinion of the reviewer this material has never before been so graphically presented at the textbook level.

On the basis of the consideration of sensory experience a treatment is offered of perceptual, imaginal, and affective experience. Attention is treated as clearness. The higher processes are considered under the respective headings of association, action, the thought processes, and the self.

The volume as a whole represents a remarkably compact and coherent view of the psychology of experience. It should be of value not only to the beginning student but to the general reader who wishes to be informed upon that type of scientific psychology which is grounded in the study of experience for its own sake.

Brown University — LEONARD CARMICHAEL

That Mind of Yours: A Psychological Analysis. By D. B. LEARY. Philadelphia, J. B. Lippincott Co., 1927. Pp. 226.

Under the title of this book the author published a series of articles in the *Buffalo Evening News* for a period of four years previous to the publication of this volume. This experience led him to believe that there is need of presenting a popular but scientific statement of the newer knowledge of psychology. The volume, then, is an attempt to lead the general reader "to see through the eyes of the modern psychologists, to get their slant on human nature, to see solutions and plans as they see them, to work with them . . . toward that better life the making of which would alone justify the delving into human depths" (p. 11). Approximately seventy-five pages are devoted to the behavior of the normal human being, and about ninety-five to abnormal behavior. In the former part the unity of the organism is emphasized and the treatment is based largely on the view of drives, instincts or tendencies, and emotions, which are not clearly differentiated. The author begins with the Lockian view of sensations as the basis of all knowledge, which is not made very consistent with the emphasis on drives, and goes up through questions of volition and responsibility. The part dealing with abnormal behavior begins with individual differences, and leans considerably toward the Freudian view. A closing chapter is devoted to wider applications. The psychologist would prefer the use of 'impulses' to that of 'outgoing messages,' and purely on the basis of experiments would disagree with the statement that beauty depends (apparently wholly) on individual experience. The volume, on the whole, is a successful piece of popularizing work, and can be strongly recommended to the general reader.

J. P.

La Philosophie de l'Art Classique. By DRAGUICHA LAPCEVIC. Paris, Félix Alcan, 1927. Pp. 124.

"The purpose of this study," as the author puts it, "is to demonstrate the necessity for a return to the spirit of antiquity, if there is to be any progress in our civilization," since modernism has made of man a being essentially unnatural and artificial as compared with the Greek. The author regards classic art in every age as the medium par excellence for presenting society with

normative ideals of humanity which are biologically correct and desirable. The book evinces familiarity with classical culture and contains here and there suggestive *aperçus*; but there is no new argument or new evidence adduced for what is in the main an acceptable thesis. At times the author seems over enthusiastic, dogmatic, needlessly repetitious, over credulous, and even rather commonplace. That the Venus of Melos represents a standard or even the ideal of perfect womanhood, may pass unchallenged; but not all students of esthetics will agree without argument that the Apollo Belvedere is the masculine counterpart of the Aphrodite (or, as Reinach prefers, the Amphitrite), nor that it is the finest product of classical sculpture. Moreover, students of biology will hardly take as a demonstration the belief of the author that the contemplation of this statue by pregnant Greek women resulted in beautifying their offspring, which seems rather a return to the letter than to the spirit of Greek science.

Vanderbilt University HERBERT SANBORN

Selected Papers on Psychoanalysis. By KARL ABRAHAM. Translated by Douglas Bryan and Alix Strachey. Introduction by Ernest Jones. London, The Hogarth Press, 1927. Pp. 527.

This volume includes the more important papers of the author arranged chronologically. They represent the orthodox psychoanalytic doctrine and show quite well the changes which have occurred in these doctrines. The first paper, published in 1907, emphasizes the sexual traumas in children whereas the later papers discuss the castration complex and similar topics.

The book forms a useful contribution to psychoanalytic literature, giving the views of one of the leaders of this group and showing quite well the manner in which his own views have developed as he continued his studies.

The introductory memoir of thirty-three pages by Ernest Jones gives an excellent account of Abraham's life and work and discusses the various contributions which Abraham has made to psychoanalysis.

Boston Psychopathic Hospital KARL M. BOWMAN

The Supplementary Reading Assignment. By CARTER V. GOOD. Baltimore, Warwick and York, 1927. Pp. ix, 227.

The writer attempted to show objectively whether it is better (from the point of view of accuracy and range of information, reproduction of thought, use of materials read in the solution of situations, and retention of ideas) to read intensively a short selection or to read extensively a much larger amount of material. Four topics were selected for reading and parallel groups of subjects were arranged. After each reading these groups were tested by means of objective examinations. In all the situations except the one dealing with the ability to reproduce ideas, the extensive method was found to be superior.

This book is of little value to psychologists but may serve as a reference in certain courses in educational psychology.

Peabody College S. C. GARRISON

On Stimulus in the Economic Life. By JOSIAH STAMP. Cambridge, Cambridge University Press, 1927. Pp. 68.

Stimulus is here defined as increment in incentive. Care is taken to distinguish increment in incentive from enlarged scope though it appears that this distinction is ordinarily ignored in economic literature. A classification of stimuli according to the character and permanency of their effects is attempted and each type is illustrated by analogies drawn from different fields of scientific inquiry. No quantitative statements concerning the relation of reaction to stimulus are found which can be carried over into the economic field. A plea is made for the discovery and utilization of stimuli which fit in with economic life and lift it permanently to higher levels of productiveness.

Kansas State College — J. C. PETERSON

Psychological Studies. By THEODOR LIPPS. Translated from the second German edition by Herbert C. Sanborn. Baltimore, Williams & Wilkins Co., 1926. Pp. 225.

This translation makes available in English Lipp's three valuable classic essays on "The Space of Visual Perception," "The Nature of Musical Consonance and Dissonance," and "The Law of Psychic Relativity and Weber's Law." These essays are too well known to need comment here. It suffices to commend this translation of them as both accurate and in good idiom.

Hobart College — FORREST LEE DIMMICK

Why We Look at Pictures. By CARL H. P. THURSTON. New York, Dodd, Mead & Co., Inc., 1926. Pp. xi, 338, 86 pl.

In the words of the author, the book is intended "as a practical aid to the enjoyment of pictures." Aesthetics, rather than psychology, constitutes the subject matter, though there are psychological discussions of color, form, etc. The book is written for the "average man" and the principles presented are made clear by frequent reference to a group of illustrations of famous paintings at the end of the book.

Geneva, N. Y. — CLAIRE C. DIMMICK

Le Progrès de la Conscience dans la Philosophie Occidentale. By LEON BRUNSCHVIGG. Paris, Félix Alcan, 1927. In two volumes. Pp. xxiii, 807.

In these volumes M. Brunschvigg sketches with a bold and sweeping brush the moving picture of the development of ethical thought and teaching in the eastern European tradition. Starting with Socrates, the author passes in review the principle problems which have agitated philosophers concerning conscience, duty, and the place of reason and intelligence in the domain of correct conduct, down to Bergson and his contemporaries. While these are philosophical volumes they are not without value to the student of psychology who is interested in the general intellectual background in which his science has had its origin and much of its development.

Indiana University — J. R. KANTOR

Recent Developments in the Social Sciences. Sociology by C. A. ELLWOOD, Anthropology by C. WISSLER, Psychology by R. H. GAULT, Cultural Geography by C. O. SAUER, Economics by J. M. CLARK, Political Science by C. E. MERRIAM, History by H. E. BARNES. Edited by E. C. Hayes. Philadelphia, J. B. Lippincott Co., 1927. Pp. vii, 427.

This book, written by seven specialists, is a comprehensive and critical review of the significant trends in the social sciences. It has a general appeal to the psychologist interested in the intellectual movement called sociology. In the chapter on psychology, however, such medical statements as "the injection of adrenalin is a means of recovery from fatigue," and "low levels of intellectual life are correlated with inadequate functioning of glands of internal secretion alone," should be taken with the same grain of salt that one takes the story that Tom Thumb, the midget's son, ate a pituitary plum and became the Giant that Jack killed.

Boston Psychopathic Hospital — S. C. ROTH

The Kingdom of the Mind. By JUNE E. DOWNEY. Introduction by Edwin E. Slosson. New York, The Macmillan Co., 1927. Pp. ix, 207.

This is the third volume of *The Young People's Shelf of Science*, edited by Slosson. It is a presentation, chiefly for boys, of a diversified group of experimentally established facts in psychology. Though a relatively large amount of space is devoted to facts of secondary interest, like the blind spot, cutaneous sensitivity spots, minor illusions, images, etc., the author shows skill in relating these facts to larger human interests. She tactfully and informally leads her young readers into methods and values of taking self-inventories. The book is sound scientifically, and will doubtless furnish profitable recreation to young people and to the popular reader generally.

Peabody College — C. W. TELFORD

Die praktische Psychologie im Wirtschaftsleben. Eine systematische und kritische Zusammenfassung des gesamten Gebietes der Wirtschafspsychotechnik. By WILHELM WEBER. Leipzig, Barth, 1927. Pp. 418.

This, the latest of the many German books dealing with industrial psychology, purports to be a thorough treatment of the subject, covering in addition to the usual topics of production, vocational guidance, fatigue, the subject of advertising and selling, and the bearing of industrial psychology on psychology in general and on the various psychologies in particular.

The discussions are copiously annotated with references to experimental literature, though practically all the references are to German works. Citations are made to tests designed for the prognosis of aptitude in about seventy-five occupations.

A section on industrial psychology in foreign countries devotes several pages to work of American psychologists but the sources from which the descriptions are drawn are obviously secondary. The chief topic treated is the threadbare one of psychology in the army. No developments in vocational guidance of a later date than the time of Parsons, 1908, are cited.

The frequency with which the word *Arbeitspsychologie* appears in German publications suggests that an English equivalent might render important service in directing the thinking and experimentation of American psychologists along the line of industrial psychology.

The book reflects very clearly the large amount of activity in this field which is taking place in Germany, and will serve as a valuable index to American readers who wish to know of these developments.

Columbia University — H. D. Kitson

Aesthetics of the Novel. By Van Meter Ames. Chicago, University of Chicago Press, 1928. Pp. ix, 221.

We admire art, this book holds, because by empathy it refreshes us. People read novels to gain vicariously what they are yearning for. It therefore becomes necessary to understand human personality if the novel is to be judged as a work of art. Moreover, the reader's character determines the novel he will select, and his reading of the selected novel itself further molds his character. The book is brilliantly written. It may be that it is too brilliantly written. The style of some of Ames' paragraphs is so dazzling that the reader is distracted by ingenious word patterns from the important thesis of the volume as a whole.

Brown University — Leonard Carmichael

An Elementary Psychology. E. D. Phillips. Revised Edition. Boston, Ginn & Co., 1927. Pp. v, 420.

The revisions in this book are not intended by the author to bring it up to date as a scientific book on psychology. Scientific technicalities and most of the content of psychology, too, are omitted. The chief emphasis is on the attempt "to stimulate young people to build their own character and wholesome relation to others." The treatment of psychological topics is general, loose, and no concern is shown for precision; the emphasis is placed on the arousal of interest. The revision includes certain of the recent contributions to psychology from this general standpoint. The reviewer doubts the helpfulness of this sort of book to the better class of college students.

J. P.

Morale et Pedagogie. By Paul Lapie. Paris, Félix Alcan, 1927. Pp. 238.

This book is a collection of articles published in the *Revue Pedagogique*, and the *Manuel Général.* This volume and a companion volume entitled "Méthodes et Directions" commemorate the work of Paul Lapie. Lapie's philosophy of education is given in papers including "The immortal flame," "The scientific miracle," "Sophisms," "Meditations on Enthusiasm," "The code of ethics of Humanity." Several papers deal with specific phases of school organization and work.

"An experiment" compares 660 compositions written by students in the Lycées with 856 written in the Écoles Normales. Among the differences in the two systems is the absence of Latin and Greek in the Écoles Normales. The quality of the compositions in the Écoles Normales is superior to that in the Lycées. A comparison of the ages of the two groups shows the Écoles Normales students to be enough older to account probably for their superiority.

North Carolina College for Women — J. A. Highsmith

NOTES AND DISCUSSIONS

Methods of Practice Equilibration

Although the student of experimental psychology will sooner or later discover the various psychophysical methods and the various methods for experimentation within the field of memory through sheer exposure to reports of individual researches, we commonly recognize a value in culling these methods from their setting in individual experiments and presenting them together in some sort of systematic arrangement. For some time I have been presenting to my students a similar classification and systematization of the methods commonly utilized in equilibrating the effects of practice. The particular methodological problem here represented has, of course, become increasingly important during these last twenty years, which have seen a wide extension of experimentation dealing primarily and frankly with capacity and performance.

When presenting this inventory of practice equilibration methods to students I illustrate each procedure by the citation of one or more actual investigations. Since the professional psychologist will readily supply his own illustrations, however, I shall omit them in these notes. Nor is much demanded here in regard to the prevalence and importance of practice effects throughout all but the simplest of psychological performances.

I. The Method of Completed Practice

The method of completed practice requires that improvement due to practice in the performance in question has ceased to occur. If this is actually the case the *practice order* in which one or another condition of experimentation is placed can safely be neglected. Insofar as order influences 'fatigue' or 'set' or some other significant variable it cannot be ignored, but so far as we are concerned merely with practice order that factor can be neglected.

Upon first thought the method of completed practice would seem to offer an ideal means of equilibrating practice effects. In pure form, however, it is almost never employed. Two of the difficulties are practical. For most types of performance the actual limit of practice is beyond experimental reach. Even where a level of achievement seems to have been reached, we know that a slight alteration in attitude is likely to bring about a resumption of improvement from practice.

A second difficulty with the method of completed practice is that even in its merely approximate forms it can be used, as a rule, only with a small number of *S*s. Many repetitions of an experiment are seldom possible with a large group. While there are in many instances matters of much greater consequence than the number of *S*s employed, there are experimental conditions peculiarly likely to be met in eccentric ways.

Then there is still another difficulty with the method of completed practice which is on quite a different level. It is a legitimate question as to what effect

alcohol or loss of sleep or irrelevant sound will have upon a given performance while that performance is still in a formative stage. In other words, if our observations were restricted to perfected habits, we should learn the susceptibility of such habits to certain influences without necessarily learning the susceptibility to the same influences of that large array of performances still manifesting practice effects.

In the conduct of investigation the method of completed practice is generally utilized in an approximate form. Enough practice is given to bring the performance in question out beyond that early period during which practice effects are most pronounced. Some additional method is then likely to be introduced to care for such practice effects as are still occurring.

II. Methods of Uncompleted Practice

(*A*) *Method of Predicted Learning.* There are occasions when, even though practice cannot for one reason or another, be continued until it has become an ineffective variable, we can survey the effects of practice over a long enough period to enable us to predict their future nature. Practice curves typically show a negative acceleration beyond which their rise is extremely gradual and fairly close to linear. If we can have a given performance repeated until it is in that stage of gradual and linear improvement, practice effects can be predicted.

There is the theoretical possibility of subtracting the influence of practice order even when the performance is still improving at a rapid rate. But this possibility, which rests upon the mathematical extrapolation of efficiency values, is certainly no more than theoretical. The variability of most practice curves is such that there is almost sure to be a better procedure than that of mathematical extrapolation. In other words, if a performance is still undergoing rapid improvement due to practice the careful investigator will postpone the insertion of his experimental differential or else take care of the factor of practice order by one of the experimental procedures yet to be described.

There is one circumstance in which the use of the method of predicted learning is exceedingly safe even though that learning is still rapid. If the experimental condition consistently works against the direction of the practice effect we may be sure that that condition is effective and we may be sure of the direction of its effectiveness. If dart throwing increases in efficiency regularly and rapidly through twenty successive days of practice and if, coincidently with the introduction of alcohol or distracting noise, efficiency on later days is at a markedly reduced level, then there need be no great doubt that the experimental condition has had a detrimental influence. In this situation the fact that a performance is still improving with practice is not an important methodological disadvantage, unless one is seeking to determine something more precise than the direction of an influence. From one point of view the sharper the rise predicted from future practice, the more reliable the test of an experimental condition. If alcohol, when administered at the time of later trials, is associated with a drop in efficiency, the sharper the rise expected without alcohol, the more confidence we may have in the general direction of our results. If, on the other hand, the experimental condition should have a generally favorable influence, then the greater the expected

effects of practice, the more difficulty there would be in discriminating between the expected effects of practice and the influence of the favorable condition.

(*B*) *Method of the control group.* The factor of practice order can be neglected if we divide our *S*s into two groups one of which, at a given point in the practice curve, performs in the presence of the experimental condition and the other of which, at the same practice order, performs under a relatively neutral condition. This method is widely employed. The practical difficulty is that ordinarily a large number of *S*s must be used in order that we may get two groups of equal capacity for the performance under examination. Of course, the use of large numbers is a certain insurance that whatever result the experiment may yield will not be a function of the peculiarity of some individual or of some small group. But again there is the disadvantage that the employment of large numbers of *S*s usually means that they can be subjected to few repetitions of the experiment.

A further difficulty with the method of the control group is complementary to one present in the method of completed practice. In that latter method it is possible to examine the influence of a given experimental condition only upon a relatively fixed habit, because only such a habit has passed through the stage of improving with practice. Since, in the method of the control group, it is usually possible to have no more than one or a very few sittings of every *S*, we must rest content with determining the influence of our experimental condition upon a relatively unperfected habit.

(*C*) *Methods of counterbalanced order.* In case it is possible to repeat the two or more conditions of experimentation, practice effects can be cared for by arranging that, on the average, the order of the conditions is the same. Let two conditions of experimentation be *A* and *B*. If these conditions appear in the order *A*, *B*, *B*, *A*, *A*, *B*, *B*, *A*, etc., the average order of the two conditions will be the same. The validity of this procedure, which might be called that of *equal average order*, depends upon the degree to which the practice curve for the performance involved is linear or uniformly curved. While absolute linearity or uniformity of curvature is certainly rare enough in practice curves, sudden flexion, such as would introduce important error into an experiment relying upon the procedure of equal average order, is also rare.

One useful check upon the procedure of equal average order is to compare the experimental conditions at different serial sections of the whole experiment. If the combined *A*s from the first and fourth orders show a marked difference from the combined *B*s of the second and third orders, and if the same relation between *A* and *B* holds for the *A*s of the fifth and eighth orders and for the *B*s of the sixth and seventh orders, and if analogous comparisons result from subsequent sections, we need not worry further as to whether the practice effect has actually been linear or uniformly curved.

Under certain circumstances the attainment of equal average order by such a regular arrangement as that described above is undesirable. In experiments upon the effects of drugs, for example, it may be essential to prevent the *S*s from detecting or anticipating the conditions of experimentation. In this situation equal average order can be attained by some such irregular arrangement as *A*, *B*, *A*, *B*, *B*, *A*, *B*, *A*.

If there is no reason to prevent the *Ss* from knowing the order of the experimental conditions and if the practice effect is very slight, one may be justified in using a simplified approximation to the procedure of equal average order. One may simply run off the conditions in the series: *A*, *B*, *A*, *B*, etc.

The general method of counterbalanced order has another procedure which demands the division of *Ss* into groups. There should be as many groups as there are conditions of experimentation. By putting these different groups through the different conditions in different order it is possible to have every condition occur once in every practice order. Let us suppose that there are three experimental conditions (*A*, *B*, *C*) three groups of *Ss*, and three experimental sittings for each group. Then the plan is as follows: Group I, *A*, *B*, *C*; Group II, *B*, *C*, *A*; and Group III, *C*, *A*, *B*. This might be called the procedure of *identical order*.

The most important assumption underlying the validity of this procedure is that the groups are such as will be equally influenced by a given amount of practice. As a matter of fact, although this procedure is a very popular one, the relative susceptibility to practice of the counterbalancing groups is seldom tested. The method is perhaps most often employed where there are as many as five or six conditions of experimentation under which circumstances the groups are almost sure to be small and, because small, of uneven ability. This does not mean, however, that the experiments employing the procedure are usually bad. If such a cycle as the one above is repeated a number of times and if the relative performances under conditions *A*, *B*, and *C* are consistent, then the possible effects of practice can safely be forgotten. In actual investigations these cycles are usually repeated. Furthermore, a preliminary practice period under neutral conditions is a frequent prelude to the experiment proper and this, itself, with its tendency to bring the *Ss* through the period of most marked practice effects, is a great safeguard.

Yale University EDWARD S. ROBINSON

ON "THE DECLINE OF PSYCHOLOGY" IN THE *AMERICAN MERCURY*

In the December 1928 number of the *American Mercury*, Grace Adams publishes an article on "The decline of psychology in America,"[1] that is so grossly inaccurate as to call for prompt and emphatic correction. Miss Adams errs both in fact and in interpretation. I shall have but little to say regarding her opinions and interpretations, though I consider them valueless and fallacious. Instead I shall chiefly confine myself to the correction of gross factual errors which might with insight and labor and with due regard for fairness and truth have been avoided. For the sake of the reader of her article, I shall take up the misstatements of fact, not in their order of importance or gravity, but in their order of appearance.

(1) Miss Adams states that five years before the founding of the laboratory of experimental psychology at Leipzig "William James, then lecturer in anatomy and physiology at Harvard, had equipped a private laboratory of his own." The laboratory in question, as a matter of fact, was housed and equipped by the University and instruction was offered to the regular students. It may have

[1] *Op. cit.*, 15, 1928, 450-454.

been informal, but it was in no sense "private." Regarding this laboratory, James wrote, "I myself, 'founded' the instruction in experimental psychology at Harvard in 1874-5, or 1876, I forget which. For a long series of years the laboratory was in two rooms of the Scientific School building."[2]

(2) Miss Adams says that the Clark laboratory was the third to come into existence in America. The records[3] show that laboratories of psychology were founded at Harvard in 1874-6, at Johns Hopkins in 1883, at Pennsylvania in 1887, at Indiana in 1888, at Wisconsin in 1888, and at Clark in 1889. The Clark laboratory was at best the sixth in order of establishment.

(3) According to Miss Adams *The American Journal of Psychology* was "patterned after the *Zeitschrift zur* [sic] *Psychologie* of Germany." The *Journal* was founded in 1887; the *Zeitschrift für Psychologie*, etc. was founded in 1890. Under those circumstances the *Journal* could hardly have been patterned after the *Zeitschrift*. The *Journal* as a matter of fact was patterned after no magazine; it was at the time of its founding unique in its field. Regarding its founding, Hall says, "There was then (in 1887) no other periodical on this subject in the English language except *Mind*, which was established in 1876 by Croom Robertson, who had told me the interesting story of its inception in a way that filled me with emulation. Both its spirit and its field were very different from the journal I intended, which I wanted to have, first of all, an exponent of experimental laboratory psychology."[4]

(4) Miss Adams informs her readers that "Cornell, on the advice of Hall, invited Edward Bradford Titchener, a young Englishman who had just received his doctorate at Leipzig, to take charge of its new department of experimental psychology." It was not on Hall's advice that Titchener was invited to Cornell University but on the advice of Frank Angell. Angell and Titchener became acquainted at Leipzig where they were students together during the year 1890-91. A lasting friendship was formed between them. In the fall of 1891 Angell joined the faculty of Cornell University and founded the psychological laboratory.[5] After one year at Cornell, Angell accepted the call to Stanford University, and recommended Titchener as his successor.[6]

(5) Until Titchener came to America he was, so far as the records show, unknown to Hall. Yet we are next told that Hall "immediately asked him to become associate editor of the *Journal*." Yes; 'immediately' after three years.

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[2]W. James, Experimental psychology in America, *Science*, 2, 1895, 626.

[3]W. O. Krohn, Facilities in experimental psychology in the colleges of the U. S., *Report U. S. Com. Educ.*, 1890-1891, 1139-1151. E. B. Delabarre, Les laboratoires de psychologie en Amérique, *Année psychol.*, 1, 1894, 209-255. C. A. Ruckmick, The history and status of psychology in the United States, this JOURNAL, 23, 1912, 517-531. [Ruckmick did not receive a reply to his questionary from the University of Pennsylvania, consequently that University was omitted from his discussion.] J. McK. Cattell, Early psychological laboratories, *Science*, 67, 1928, 542-548.

[4]G. S. Hall, The American Journal of Psychology, this JOURNAL, 32, 1921, 1-3.

[5]C. A. Ruckmick, *op. cit.*, 520. J. G. Schurman, Report of the dean of the Sage school of philosophy, *Annual Report of the President of Cornell University 1891-1892, Appendix VI*, 1892, 30.

[6]Frank Angell, Titchener at Leipzig, *J. Gen. Psychol.*, 1, 1928, 198. Cf., also, E. G. Boring, Edward Bradford Titchener, this JOURNAL, 38, 1927, 493.

Titchener took up his duties at Cornell in September 1892; he became an assistant editor of the *Journal*, as the title pages of the *Journal* indicate, in October 1895.

(6) The statement is made that "Titchener's works have been translated in more foreign languages than those of any other psychologist." What Miss Adams's evidence is I do not know. Existent bibliographies of Titchener[7] and of Wundt[8] show that Titchener's works were translated into seven languages and that Wundt's were translated into eight.

(7) In order to assist her in the establishment of the thesis that Titchener "was an unqualified German," Miss Adams says that under his editorship the book reviews of the *Journal* were almost without exception concerned with German publications." Titchener was sole responsible editor of the *Journal* for five years, 1921-1925. During that period 110 books were reviewed. Of these 22 or 20% were German.

(8) Miss Adams writes of the *Journal of Religious Psychology and Education*, founded by Hall in 1904, as his second publishing venture. It was his third. He founded the *Pedagogical Seminary* in January 1891.

(9) Regarding the "birth of behaviorism," we are informed that it resulted from Watson's revolt against the teaching of James's *Principles*. Watson inveighs, in his pioneering articles on Behaviorism,[9] against the psychological method of introspection. His 'revolt' is, as he says (and he should know), against the view that psychology is the study of consciousness. "The time seems to have come," he says in his first article, "when psychology must discard all reference to consciousness." "My psychological quarrel is not with the systematic and structural psychologist alone," but it is also with the functional psychologist who has appeared during "the last fifteen years," *i.e.* since 1898. James's *Principles* was published in 1890.

(10) We are then informed that "the robustness of Watson's assertions soon won him the professorship at the Johns Hopkins." A glance in *Who's Who* would have shown Miss Adams that Watson obtained his full chair at Johns Hopkins University in 1908, five years before his assertions were robust enough to see the light of public utterance and print.

(11) Miss Adams writes that when McDougall came to Harvard, "the American Psychological Association quickly and eagerly elected him its president." McDougall has never been president of the American Psychological Association, nor has he held any office within the association with which the presidency might be confused. He was, to be sure, president of the Ameri-

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[7]W. S. Foster, A bibliography of the published writings of Edward Bradford Titchener 1889-1917, *Titchener Commemorative Volume*, 1917, 323-337. K. M. Dallenbach, Bibliography of the writings of Edward Bradford Titchener 1917-1927, this JOURNAL, 40, 1928, 121-125.

[8]E. B. Titchener and L. R. Geissler, A bibliography of the scientific writings of Wilhelm Wundt, this JOURNAL, 19, 1908, 541-556. Three supplementary lists were published in collaboration with Geissler, three with W. S. Foster and one with S. Feldman.

[9]J. B. Watson, Psychology as the behaviorist views it, *Psychol. Rev.*, 20, 1913, 158-177; Image and affection in behavior, *J. Phil. Psychol. & Sci. Meth.*, 10, 1913, 421-428.

can Society for Psychical Research in 1921 and 1922.[10] Is it possible that Miss Adams—who writes in the tone of an informed psychologist—could have mistaken these utterly unlike offices?

(12, 13) Again it is said in the pages of the *American Mercury* that "some time before his death Titchener . . . withdrew his support from the American Psychological Association. He then invited those members whose attitude and seriousness he approved to meet with him and discuss their experimental problems. These conferences grew into annual meetings." Two errors of fact appear in those lines. First, one cannot withdraw what has never been given. Titchener never gave the Association his support. He did not attend the meetings of the Association, not even the meetings which were held in 1925 on his own campus. Secondly, his withdrawal and the organization of an experimental conference are declared to have occurred "some time before his death." That seems obvious! The phrase, if it means anything, must mean within comparatively recent times before his death, say 2, 3, 5 or 10 years. The "some time" is over 23 years, as the experimental conferences to which Miss Adams refers were organized in 1904.[11]

(14) The conferences thus organized had, we are falsely informed, "no official name, no elections, and no formal papers." As a matter of general knowledge the conferences were always known as the meetings of the Experimental Psychologists,[12] and formal papers were the rule until the 11th annual meeting in 1914, and they were, on special occasions, read after that, even as late as the meeting of 1925.

(15) "The purpose of the conferences," Miss Adams explains, "was to get exact experimental work done and he [Titchener] personally saw to it that it was done. Such departments as that at Columbia were ignored as though they did not exist." The reference to Titchener is grossly exaggerated and the reference to Columbia is absolutely untrue. The department of psychology at Columbia was one of the ten departments that participated in the organization of the group, and representatives from the Columbia laboratory have, with one or two exceptions, attended every meeting.

(16) Miss Adams must, in addition to these misstatements of fact, be charged with many half-truths. Such statements as "The undergraduate at Yale, mapping out his yearly courses, can choose between the psychology of advertising or the psychology of business," "the candidate for a Ph. D. degree [at Chicago] can find congenial work in the psychology of business, the psychology of personality, or the theory of statistics," are misleading. The reader of these lines who does not know the true situation at Yale and Chicago would derive from them a false notion of the status of psychology at these two universities.

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[10]Cf. *J. Amer. Soc. Psychical Res.*, 15, 1921, 315ff.

[11]E. G. Boring, *op. cit.*, 498.

[12]Titchener always referred to it thus both in print (cf. E. B. Titchener, Experimental psychology: a retrospect, this JOURNAL, 36, 1925, 313, footnote 1) and in speech; and the proceedings that were published of the meetings of the early conferences always bore that name (cf. this JOURNAL, 18, 1907, 383-388; 19, 1908, 288; 20, 1909, 471, etc.).

These errors may be regarded by some as trivial and as irrelevant to the central theme of Miss Adams's article. But they are not trivial, and they are relevant to the author's main contentions. The accuracy of a writer is a measure of the value of his opinions.

A good deal could be said about the value of Miss Adams's interpretation of the matters which she has thus falsely and unfairly presented. I shall content myself, however, with the discussion of but one of her opinions—and that one is the central theme of her article, viz. that American psychology is on the decline.

It is of course natural and, perhaps, on the whole, salutary that there should be a 'golden age.' The faltering steps of the 'trail blazers' seem giant strides to the graduate students and to those younger psychologists who have yet to gain a perspective in their field. The task of evaluating and comparing the accomplishments of one age with those of another is one of the most difficult with which the scientific historian has to deal. It should not lightly be undertaken. Fortunately two of the 'trail blazers' of psychology in America, two of the giants from the mythical 'golden age,' have recently given us their opinions regarding the present status of psychology. They both have the right to be heard.

Titchener said in 1925,[12] "Experimental psychology came of excellent stock, . . . its growth has been, on the whole, steady and assured. The question that remains is—What, in the year of grace 1925, is its mental age? The question is one, I take it, that has attracted us all . . . It is, of course, a question that we cannot properly answer at all, because—if I may use the figure—we stand somewhere within our own perspective, and therefore cannot judge either how far we have come or how far, to attain the status of the adult, we have yet to go. . . . We have, I think without question, passed from infancy into childhood. If . . . the intellect is most alert and most capable at the age of sixteen, we may congratulate ourselves that experimental psychology is nearing that critical point, and may expect far better things from it in the near future than have been accomplished in the past."

Cattell said in 1927,[14] "With the publication of James's *Principles of Psychology* in 1890, the opening of the laboratories at Harvard, Yale and Cornell in 1892, and the establishment of the American Psychological Association in the same year, the earlier period of psychology in American may be closed. The few survivors may look back upon it as the golden age of our science, but that is doubtless due only to the presbyopia that obscures the vision of objects near at hand. In the thirty-five years that have since passed, the number of our workers in psychology has increased to an extent perhaps without parallel in any other country or in any other science."

These masters of Psychology speak of its *rise*. Miss Adams imagines its "decline."

This note is obviously out of place in a scientific journal. It not only takes space that should be devoted to other matters, but it also fails in its main

[12] E. B. Titchener, Experimental psychology: a retrospect, this JOURNAL, 36, 1925, 322 f.

[14] J. McK. Cattell, Early psychological laboratories, *Science*, 67, 1928, 547 f.

objective, since few of the readers of Miss Adams's article in the *American Mercury* will ever see or learn of this correction. The false idea that the readers of that magazine may have gained of the present status of psychology will therefore persist unchallenged. This note should have been published in the *American Mercury;* but that avenue of publication was closed against it. Its editor wrote as follows, in reply to a letter asking him whether he would, in view of the great number of factual errors occurring in Miss Adams's article, publish a correction.

"A number of criticisms of Doctor Adams' article have been received, and I have sent several of them to her. She reports that, save on a few minor points, they are in conflict with one another. Thus I fear that it would be unwise for the *American Mercury* to go into the controversy."

Miss Adams replied as follows to a critical letter that I wrote to her. "Last week Mr. Mencken received long protests about my article from Profs. Ferberger [Fernberger] and Boring which contradicted each other so thoroughly that after they were carefully compared the glowing errors which they claimed they were pointing out were no where to be found. I read your letter with interest, hoping it would clear up the confusion, but was greatly disappointed to find that it only added to it. . . . The only point on which you three are agreed is that William McDougall has never been president of the American Psychological Association, but there is such a difference of opinion in the letters about his relations with this august body that I feel I should wait for McDougall's personal denial before I admit even that an error. The only thing that seems certain from the controversy is that we psychologists are woefully ignorant about our own subject and find it difficult to distinguish an opinion from a fact."

Since Miss Adams named the other critics of her article, I took the opportunity to test further her accuracy of report by direct reference to the "protests" of Professor Fernberger and Professor Boring. A careful comparison of their "protests" with each other and with my own reveals absolutely no disagreement of fact.

The surprising thing about all this is, not that Miss Adams should have found factual disagreement where none exists, but that she should have been made the judge of the criticisms of her article, and that the editor of the *American Mercury*, who states (December 1928, p. 407) that his journal "simply aims to give. . . accurate and realistic news of what is going on in the country," should have accepted her report as final.

KARL M. DALLENBACH

DR. YOUNG ON TRAINED OBSERVERS

P. T. Young, in a problem reported under the title, "Studies in Affective Psychology,"[1] repeated a part of a problem previously reported by me upon practically the same subject.[2] In his work Young used one of the *Os* who had served in my problem and other *Os* who had not served in other problems on

[1] This JOURNAL, 38, 1927, 157-193.

[2] J. P. Nafe, An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.

this subject and whose training differed materially from that of the 'trained' *O*. Young concludes, as I understand him, that the results of both of these problems depended upon the training of the *O*s and such training he holds to be prejudicial rather than effective in discovering the facts. Young says in part, "A 'trained' *O* is not necessarily one practiced in psychological observation. The important factor in training is the psychological education or indoctrination to which the *O* has been exposed. The terms of description which an *O* has acquired, the psychological facts he knows, and his theoretical views very largely determine the type of report."

I agree with Young that previous training is important in determining the *type* of report to be expected but I do not agree that it is even a factor in determining *the report*, that is, the facts reported. He points to the methods of other sciences and I am willing to put my answer in similar terms.

In microscopy we do not turn an instrument over to one who is naïve in regard to either the use of the instrument or the nature of what one is expected to find. But no one will be found who maintains that the results obtained by one skilled in the use of the microscope and familiar with his subject matter are not as trustworthy as those of the naïve student. It must be evident that in any ordinary problem in the physical or biological sciences the *O* who knows what kind of phenomena he is looking for is at an advantage rather than at any disadvantage and his results are more, rather than less, valuable.

Young suggests the superior value of such responses as "I don't like it" which, he says, is the type of response to be expected from a naïve *O* under the conditions common to both problems. What is the answer? Obviously, if he wishes to determine whether or not the *O* likes the experience the answer has a value but if he is asking for a report upon the psychological experience, *as stated in both his and my instructions*, such an answer is valueless.

Suppose the *O* using the microscope, at the conclusion of his effort, gives the information, as a naïve *O* might, "I don't like it." Such an answer could have no importance for one waiting for a description of the matter under the glass.

Of what greater value is such a reply to one investigating the 'content of consciousness?' None. The *O* is instructed to report the experience. Some *O*s know enough to do so and to omit casual remarks and such knowledge depends upon training. The reports of such *O*s are not, it seems to me, to be considered invalid because prejudiced.

Clark University JOHN P. NAFE

MR. HOVEY ON DISTRACTION

In a report on "Effects of general distraction on the higher thought processes,"[1] Mr. Hovey states "that distraction is not a serious handicap to the group as a whole during 19 min. of concentrated thinking." Upon studying the figures presented I am unable to find substantiation for this conclusion and, indeed, consider that the opposite condition is indicated. Here is the situation and the argument. The 'experiment-group' works Army Alpha

[1] This JOURNAL, 40, 1928, 588.

problems under conditions of relative quietness. About one and one half months later this group again works on Army Alpha problems (another form), this time under conditions of great noise. There is reported a gain of 6.4 points in the case of the second condition. This looks as if the noise condition actually helped efficiency. The gain, however, might be due to practice on the first form. The investigator wisely conducted a control experiment with a 'control-group.' This group worked on two occasions with the same materials, but a condition of quietness obtained on each occasion. A gain of 10.1 points was found in the case of the second work-period. This gain was, then, due to maturation and practice effects. Now for the argument. If the experiment-group had shown a gain of more than 10.1 points there would have been evidence of increase in efficiency due to the noise, over and above practice and maturation. If the gain in the case of the experiment-group had been just 10.1 points then there would have been evidence of 'just not any detrimental influence of the noise.' If the gain for the experiment-group had been less than 10.1 (as was actually the case), the evidence would be that the noise actually played a detrimental rôle, although it could not eliminate the practice effect.

Incidentally, one may wonder when experimenters will cease speaking of 'distractors.' Why not speak simply of 'sense stimuli' until it has been proven that they are really distractors. In which case the article would be headed, "Effects of sense-stimuli on the higher thought processes."

College of the City of Detroit E. B. Skaggs

Research Interests of American Psychologists

Early in September, the Program Committee of the American Psychological Association sent a list of topics to the 700-odd members and associates of the Association as suggestions for Round Table Discussions at the 1928 meeting. The list contained 36 topics—space was left for the addition of other topics that might be desired. The members and associates were asked to check all the topics in which they were interested and in the discussion of which they would participate. A total of 212 replies was received. Individuals checked from 1 to 20 topics. In all, 1019 topics were checked. Only 22 topics were added to those given in the lists.

The number of times the different topics were checked is, in order of preference, as follows: Personality, 70; First Course in Psychology, 55; Mental Tests, 52; Motivation, 49; Childhood and Adolescence, 46; Feeling and Emotion, 44; Abnormal Psychology, 41; Individual Differences, 37; Methods of Learning, 36; Requirements for the Ph.D. Degree, 30; Facial and Postural Expression of Emotion, 30; Consciousness and Behavior, 29; Relation of Nature and Nurture, 28; Motor Learning, 28; Reflexes and Instinct, 27; Psychophysical Measurement Methods, 23; Statistical Methods, 22; Psychogalvanic Reflex Experiment, 22; Thought, 21; Relation of Anthropology and Psychology, 20; Sleep, Dreams, Hypnosis, 20; Animal Ideation, 20; Introspection as Method, 19; Mind and Body, 19; Space Perception, 19; Nature of Attention, 19; The Correlation Methods, 18; Vision, 18; Language, 18; Educational Tests, 18; Animal Behavior, 17; Esthetics, 16; Audition, 15; Seen Movement, 11; and Taste and Smell, 6.

The results indicate a strong technological interest among American psychologists. It is true that the problems of Personality (1st) and of Motivation (5th) have a theoretical as well as an applied interest; but the individuals checking these two topics more frequently checked other applied than other theoretical and experimental topics. It is also of interest that the topic the First Course in Psychology should stand second in the list, indicating a widespread desire for standardization in this direction. Of the experimental problems, those concerned with emotion show outstanding interest—Feeling and Emotion standing 7th, Facial and Postural Expression of Emotion standing 12th, and the Psychogalvanic Reflex Experiment standing 19th. Problems of statistical methodology fall in the middle of the list—the Psychophysical Measurement Methods ranking 17th, Statistical Methods ranking 18th, and the Correlation Methods ranking 29th. The problems of Sensation and Perception are to be found at the end of the lists along with Language, Esthetics, and Educational Tests.

University of Pennsylvania SAMUEL W. FERNBERGER

ERRATA

A number of printer's errors occurred in the review section of the October number of the JOURNAL. Most of them fortunately will be immediately recognized; but one occurred in the review of East's *Heredity and Human Affairs* (p. 646, sixth line of the review) which entirely changed the meaning of the sentence and which therefore must be corrected. Copy read, "The psychologist can only wish that these data had been somewhat more representative and up to date;" but the compositor set it, "The psychologist can hardly wish," etc. The change of "only" into "hardly" is curious, for the copy was perfectly clear.

J. P.